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# XIV. *The Dynamics of Pianoforte "Touch."* *By Prof.* G. H. Bryan, *Sc.D.*, *F.R.S.*

Received February 6, 1912.

The problem of obtaining formulæ for the vibrations of a stretched string when struck by a pianoforte hammer is approached from two very different points of view by Helmholtz and Kaufmann, and there is a correspondingly wide difference in the conclusions to which the investigations lead.

Helmholtz starts with the assumption that the force exerted by the hammer on the string is given by an expression of the form $F \sin pt$, lasting from $pt=0$ to $pt=\pi$, and he applies Fourier's method to determine the amplitudes of the various vibrations set up. While with $p$ constant these amplitudes are all proportional to F, the maximum value of the exciting force, a variation in the value of $p$ will manifest itself in a change in the *quality of tone* as dependent on the relative intensities of the fundamental tone and its harmonics.

Kaufmann, on the other hand, bases his investigation mainly on the *functional* solutions of the differential equation of wave propagation along a stretched wire. He thus obtains a differential equation for the motion of the hammer. In his analysis he neglects all such forces as those due to the weight of the hammer, and thus arrives at the conclusion that if the string is indefinitely extended in both directions the hammer will never leave it—a conclusion which ought to suggest the obvious inference that it is not justifiable to leave out the weight of the hammer and similar forces without further investigation. Kaufmann's assumptions necessarily lead to the conclusion that so long as the same string is struck by the same hammer at the same point the *quality of the sound* emitted will always be the same. A general discussion is, however, given as to the influence of elasticity of the hammer on the resulting quality of tone as determined by the relative intensities of the harmonics.

Now there are good reasons for believing that the *quality* of the tones produced when playing the piano is capable of being varied not only, sometimes, by playing louder or softer but also by other differences in playing commonly associated with the somewhat ill-understood term "touch." The difference between the delicate or brilliant touch of the professional

pianist and the dull heavy thumping of the schoolgirl may doubtless be accounted for to a large measure by judgment or want of judgment in the loudness with which the notes are struck, proper or improper use of the loud pedal, small deviations from uniformity in the lengths of different notes or the presence or absence of ornaments on the top of the piano. But, in addition to all these differences, it will probably be generally admitted that there are other differences which seem to be due to the varying *elasticity* of the performer's fingers, and it is certain that many people training as pianists attend to the physical development of the muscles of their hands to an extent that would hardly be necessary if it were merely a question of regulating the total strength of the blow delivered to any key of the pianoforte.

The growing popularity of pneumatic piano-players used in conjunction with perforated rolls renders it desirable that the dynamics of pianoforte touch should be examined in greater detail than has been done either by Helmholtz or by Kaufmann. It is very generally admitted or assumed that there is a certain element missing in the performance of these machines, namely, the touch of the human fingers; and certainly the average piano-player of commerce as usually played has very little resemblance whatever to the interpretation of the professional pianist. Indeed, as applied to piano-players in general, the unfavourable opinion expressed in the "Encyclopædia Britannica" is quite a fair criticism.

A good piano-player in the hands of an experienced performer certainly cannot be described as a *mechanical musical instrument*. But it will often be found that if the keys of the pianoforte are struck with the fingers, tones are emitted which cannot be exactly reproduced by any manipulation of the piano-player, however skilful. The difference seems to be in the *quality* of the sound rather than in its loudness or softness, and if this view is correct, Kaufmann's theory is not sufficient to account for the difference.

Now there are many people who for their enjoyment of good music have to depend largely on getting the greatest possible range of effects out of a first-class piano-player, and it thus becomes interesting to examine more closely the extent to which *differences of touch can be made to cause differences in the quality of individual notes.*

The existence of any such effect obviously suggests the assumption that the impressed forces acting on the hammer so

far from being negligible, as Kaufmann assumes, are capable of considerably modifying the action of the hammer during the short interval of time that it is in contact with the string, and one such modification would be a lengthening or shortening of the duration of contact. If the differences are really independent of the loudness of the note, they can only be controlled by the pianist if he is able to produce time-variations of the pressure of his finger on the keys of the piano while they are being depressed, and it is certain that such time-variations would naturally result from variations in the elasticity of the performer's fingers, such as could be caused by a tightening or slackening of the muscles. At the same time it is very difficult to find a satisfactory explanation on dynamical principles of the relation between cause and effect, and in this connection the dynamics of pianoforte touch presents greater difficulties than the corresponding investigation for other musical instruments, the reason being the shortness of the time-interval during which the notes are excited. It is easy enough to see how the character of the tones of a violin might be altered by varying the pressure or velocity of the bow, or the elasticity of the hand drawing it, and it would not be hard to reproduce these variations in a purely mechanical experiment.

In the case of a well-adjusted horizontal grand piano, if a key is slowly depressed the hammers can be made to nearly touch the wires. If the hammer actually press against the wires the note is said to be "blocked," and fails to sound properly. It does not follow that while the hammer is touching the string no forces are impressed on it through the check action, but it does follow that these forces are not sufficiently great to support the weight of the hammer since its position of equilibrium is below the string, and the view that their effect is appreciable is directly contrary to Kaufmann's hypothesis.

In a subsequent communication I propose to investigate the differential equations of motion of the hammer in the more general case assuming various different expressions for the impressed forces acting on it. Before doing so I propose to describe a simple apparatus by means of which differences of pianoforte touch can be readily reproduced and the dynamical actions associated with them made the subject of qualitative observation.

This apparatus (Fig. 1) consists of a long horizontal lever fixed in front of the usual levers of a pneumatic player of the ordinary "Standard" type, and operating directly on the

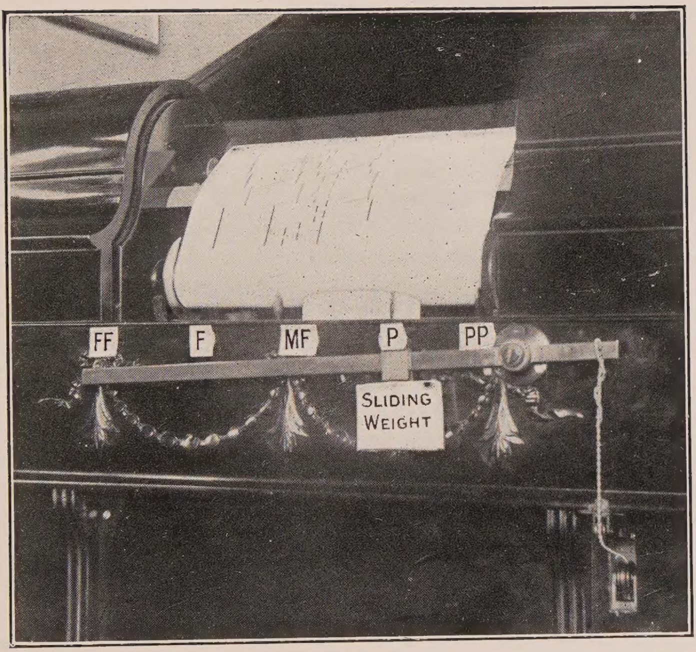
auxiliary regulating bellows or wind chamber. This lever carries a sliding weight (about $12\frac{1}{2}$ oz.) and supersedes the ordinary spring. While in the commercial player, the collapse of the auxiliary bellows is regulated only by the elasticity of the spring, which remains constant, the collapsing tension can now be varied in several ways. It is reduced by shifting the sliding weight to the right, increased by shifting it to the left, and thus it is possible to produce very rapid time-variations of the pneumatic tension reproducing the conditions that prevail when a pianist strikes the keys in different ways apart from the mere question of loudness or softness in playing. These variations are further accentuated by applying hand-pressure directly to the lever, thus causing the actual touch of the human hands to govern the variations of pneumatic tension and so to be transmitted directly to the keys of the piano.

It is well worth while to take a good deal of trouble in trying different ways of manipulating such a lever and comparing the results with the effects produced by a good pianist, as in this way a great deal can be learnt about the dynamical actions employed in producing these effects. After a little such practice it is interesting to disconnect the lever and replace the usual spring.

If my experience is any guide, the result will be disappointment, and the effects previously regarded as satisfactory will be found to be lacking in many respects. Indeed, after a very rough-and-ready preliminary experiment with sticks and strings and a kitchen weight, I found it impossible to obtain the same satisfaction with the old arrangement that I had experienced for 10 years previously.

The advantage of this arrangement is that it enables the dynamical actions to be studied by the same person who ordinarily uses the piano-player and it thus eliminates differences due to individuality which render difficult a comparison of the results produced by *different* performers, one operating with fingers and the other employing the modern pneumatic mechanism.

By merely setting the sliding weight against the letters corresponding to the expression marks in the music, the result is increased breadth of contrast, the soft passages being less dull and the loud ones less thin. By moving the sliding weight independently of the pedalling, variations of *touch* are at once obtained bearing a close resemblance to those of the professional pianist. With the weight at *pp* and strong pedalling,




[Patent applied for.]

APPARATUS FOR CONTROLLING TOUCH IN A PNEUMATIC PIANO PLAYER.

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a very "metallic" effect is produced and the treble parts predominate over the bass. With the weight at *ff* and light pedalling, softer and richer effects are produced, the bass parts now coming into prominence. In this way it is interesting to attempt a pianoforte rendering of organ music and to produce contrasts suggestive of a two-manual organ.

With regard to what can be done by hand control the "metallic" tone is greatly increased by depressing the lever and allowing it to sharply fly up again, whereas the vigorous bass effects are improved by heavily depressing the lever. A sudden jerk up from below in a loud passage often saves a treble note from being drowned by the bass.

The most noticeable feature of these experiments is the very marked differentiation that it is possible to produce between notes in different parts of the keyboard. This differentiation is particularly noticeable in the case of long chords extending over a considerable length of the keyboard; in such cases the intensities of the treble and bass parts can be varied quite independently of each other. But even in comparatively short chords it is often possible to appreciably accentuate certain notes of the chord and to vary the effect at will.

These results are due to the fact that different notes of the keyboard require different kinds of touch to bring out their maximum effect, these differences depending on time variations of the pressure applied to the keys of the piano. A short, sharp pressure produces its greatest effect at the treble end leaving the bass notes unaffected; a long, heavy pressure brings out the bass notes without affecting the treble; indeed, if we use the term "exposure" to denote the time interval during which the pressure is effective in exciting any particular note, it will be found with a little experience that the correct exposure varies continuously from one end of the keyboard to the other. It is possible with practice to judge exactly the exposure necessary to give prominence to notes in any desired part of the scale, and thus to reproduce the great majority of effects which a good professional pianist produces in accentuating particular notes of chords. To obtain the maximum enjoyment out of a piano-player it is necessary to know the feel of the touch of every note on the keyboard and this is by no means so difficult as it appears.

The actual "exposures" in the case of a piano-player are not so short as might be supposed. I have frequently failed to bring out notes in the lower part of the treble clef through

under exposure. In the case where the touch is controlled by the sliding weight it often suffices to place this opposite the part of the music roll on which are the notes which have to be accentuated.

An explanation of these effects is doubtless largely to be sought in the varying masses of the pianoforte hammers, which cause them to undergo different accelerations when the same force is applied to them by a common pneumatic pressure (or rather tension). The lighter hammers rise more quickly, and when they are released it will make all the difference whether the heavier ones are firmly pressed up or the pressure on them is suddenly reduced.

It still remains, however, problematical whether the effects are entirely due to this cause or whether the length of time that the hammers are in contact with the strings does not undergo a corresponding variation, in which case there will be a certain duration of contact that will give the maximum effect to a particular note as is provided for on Helmholtz's but not on Kaufmann's theory. Further, it is hardly possible by playing *chords* to judge of the existence or non-existence of differences in the quality of individual notes. For this purpose it is necessary to select a passage containing single notes only. The test is difficult to perform even with the present apparatus, for several reasons. When notes are struck loud the *impression* is certainly produced that the harmonics are more brilliant as a general rule. But this effect might be due to peculiarities in the sense of hearing. On the other hand, it is not easy to vary the action without altering the loudness, and it is very easy to fail. To some people a note struck on the piano sounds louder or softer and nothing else. On the other hand I have found that most people notice marked differences of quality in the notes of a solo passage produced by shifting the sliding weight on the lever although some of them state they have no ear for music.

Personally I should have not the slightest doubt on the matter so far as it is capable of being decided by observation. Indeed, I would almost be prepared to go so far as to say that piano playing, whether by hand or pneumatic action, must necessarily sound rather dull and mechanical unless individual control is capable of influencing the quality as well as the intensity of particular notes. But when we attempt to give a theoretical explanation, to apply the principles of dynamics and to write down equations of motion, difficulties occur.

Taking the case of the horizontal grand piano, we have seen that if impressed forces act on the hammer during its contact with the wire, these forces must be less than the weight of the hammer. In order to form an estimate of the effect of such forces, I have modified Kaufmann's equations by supposing the hammer to be subject to the downward acceleration of gravity, taking $g=981$. The results at first appear very promising, and they indicate that, in the case of a string struck near the middle, gravity may have an important effect on the duration of contact. In the case of a string struck near one end, the effect of gravity is much more limited. After a wave has had time to travel from the hammer to the near end and back again, the terms depending on gravity disappear from the equations of motion, and the initial effects appear, so far as the calculations go at present, to be far too small to produce an appreciable result. It is true that the striking velocities assumed by Kaufmann are largely in excess of those required for artistic piano playing. I find that for loud passages a striking velocity of 150 cm./sec. is quite sufficient, for soft passages 30 cm./sec. is more than enough ; such a velocity would be just sufficient to carry the hammer 5 mm. above the level of the wire under gravity. For really delicate playing an even smaller velocity is needed. Nevertheless, the equations do not at present seem likely to afford the required explanations. In the case of the vertical piano, the position of equilibrium of the hammers is much further from the strings and the effect of gravity much smaller. It is clear that we must either abandon our belief in the effects of "touch" on "quality of tone" or seek an explanation of a more general character.

The case is somewhat different when the elasticity of the hammer, where it touches the wire, is taken into account. From Kaufmann's extreme case of an inelastic hammer, we may pass to the opposite extreme of an elastic hammer striking a fixed rigid wire. In this case the expression for the force will assume Helmholtz's form. If, however, the hammer be also acted on by a downward acceleration due to gravity the expression for the exciting force will assume the form $F \sin pt -$ constant, so that the duration of contact will be reduced and will become shorter as the striking velocity is decreased.

It should be mentioned that some of the inferior piano-players and music rolls are not well suited for showing differences of pianoforte touch. Many such instruments have separate treble and bass buttons, so that it is difficult to get

a continuous differentiation between various parts of the scale. Others have large springy reservoir bellows, which collapse when a loud effect is required, and come smashing out on the soft passages which follow. In many music rolls, too, the chords are badly ranged. If the treble notes are even $\frac{1}{50}$ in. before or behind the bass ones, as is often the case, any attempt at differentiation is almost certain to fail and the usual dull mechanical playing is the result. Apart from these difficulties, there is an essential difference between sheet music and music rolls, the former being divided into bars, with the number of beats in each bar indicated at the beginning, while the latter possess no such indications. This feature alone renders it impossible for a person without a thorough knowledge of the music to rival the pianist of even limited experience.

Most modern inventions, such as the aeroplane, motor car, kinematograph and gramophone have been the subject of discussion in scientific and technical journals, but hitherto the pneumatically played piano has been conspicuous by its absence, So far as my experience goes there are, however, a good many people who are quite willing to take a little trouble in extending their power of control over these instruments, and under these circumstances a discussion on the dynamics of pianoforte touch may not be altogether out of place in a scientific body like the Physical Society.

## ABSTRACT.

The author discussed Helmholtz's and Kaufmann's theories of the vibrations of a pianoforte wire excited by impact, with special reference to the effects obtainable with the modern pneumatical piano-players and player-pianos, and the common widespread belief that these can never reproduce the touch of the human fingers. While the rendering of many commercial piano-players in the hands of an average performer bears little resemblance to the performance of a professional pianist, the author finds that there is generally believed to be a certain element missing even in music played by a skilled performer on a first-class modern piano-player, this missing element being commonly associated with what is described as "touch." In view of the great value of piano-players to lovers of music, it thus becomes interesting to examine more closely what is meant by "touch," and whether it is capable of being reproduced to a greater extent than hitherto upon pneumatically controlled pianos.

The question turns very largely on the extent, if any, to which the *quality* of individual notes can be varied by striking the notes in different ways. Such a possibility involves the inferences that (*a*) the intensities of the fundamental tone and its several harmonics are capable of independent variation; (*b*) these variations can only be produced by varying the behaviour of the pianoforte hammer while

it is in contact with the string, for example, by lengthening or shortening the duration of contact; (c) such an effect can only be produced by rapid time variations of the pressure applied to the keys while they are being depressed—*e.g.*, by a fairly rapid decrease or increase of pressure produced by smartly striking or heavily pressing on the key.

The author finds that Kaufmann's investigation fails to account for any such effects, and that difficulties arise even when the equations are modified so as to take account of impressed forces on the hammer comparable with those due to gravity. On the other hand, he describes experiments which appear to indicate beyond all reasonable doubt the existence of such effects of "touch," and which certainly demonstrate the possibility of reproducing them by means of the modern "pneumatic" instrument. For this purpose the author's piano-player, which is a first-class instrument of the "Standard" type, but *with the whole keyboard under one common control*, was fitted with an "auxiliary lever" for which a patent application has been filed. This lever operates directly on the face of the auxiliary regulating bellows, and the air-tension in the bellows can be regulated by means of a sliding weight placed on the lever, or by applying hand pressure to the lever itself. In this way *the touch of the human hand can be transmitted directly to the keys of the piano.* So far as the experiments go, they indicate that even if the lever is worked in conjunction with suitable expression marks, as could be done by a person of moderate experience, increased breadth of contrast is obtained. While by varying the position of the load independently of the pedalling a variety of dynamical effects can be produced, which can further be increased by hand control.

A short, sharp pressure produces a bright ringing treble with a light bass, a sustained pressure produces a rich bass with a soft treble; the general character of the tone being suitably described as "metallic" in the first case and "woody" in the second. A very conspicuous feature of these experiments is the marked differentiation which they show between notes in different parts of the scale, especially in chords, the notes of which are accurately ranged (as is unfortunately often not the case in music rolls). The duration of the pressure required to produce the maximum effect on a particular note of the piano varies continuously from the treble to the bass end, being least in the treble and greatest in the bass, and by means of this natural or dynamical differentiation notes in a particular part of a chord at any part of the scale can be accented independently of the rest.

Whether it is possible to vary the quality of individual notes is a point that can only be tested by playing single notes as opposed to chords. The differences that can be effected can only be noticed by a trained ear; in the author's experiments it has been found that some persons notice *very marked* differences, others notice very slight differences, others no differences at all. The differences are probably as conspicuous as those between a stopped string and a harmonic on the violin. It is not always easy to produce these differences for purposes of demonstration, though it is often easier to do so in the course of playing through a suitable composition. In any case the author finds that the effects can be obtained more easily with a pneumatic player fitted with auxiliary lever than in striking the keys with fingers. When the lever is disconnected the change

observed affords some indication of the origin of the popular belief in the limitations of the pneumatically played piano.

Unlike the gramophone, aeroplane, motor car and cinematograph, the modern piano-player has been conspicuous by the absence of reference to it in scientific and technical journals. The present Paper, which arises out of an attempt to obtain a closer degree of approximation to *playing the piano* with these instruments indicates that they open up some interesting problems in the study of acoustics.

## DISCUSSION.

Mr. A. Campbell, speaking as a musician, said that Prof. Bryan's invention largely increased the capabilities of the mechanical piano-player, as it gave the person who guided the machine the power of accenting particular notes at any part of the keyboard. If the *loudness be kept constant* the differences in quality of tone that could be obtained from a piano were quite infinitesimal compared with the difference given by two pipes of different stops on an organ, and were of negligible value in musical art, although their importance was so commonly magnified by fancy and by professional humbug. Piano music had to depend for its variety on factors other than difference in tone quality. The comparative constancy in tone quality could, no doubt, be demonstrated objectively by means of phonautograph records. Referring to the general question of mechanism in relation to art, he considered that the use of mechanical players tended to reduce the number of persons who acquire a real knowledge of music by learning to read it by voice or instrument in the ordinary way, and hence such machines must inevitably prove destructive to musical art.

Dr. W. H. Eccles entirely disagreed with Mr. Campbell, and considered the difference was most marked, and that Prof. Bryan's lever made a great difference in the contrast obtainable ; but he thought it would be an improvement to be able to make the soft passages softer.

The President remarked that the explanation of the difference of tone of a note according as to the manner in which it was struck—such as, for instance, between the touch of a learner and a skilled player—must interest many of even of those who were not musical.

Mr. G. H. Berry (communicated remarks), as a practical pianoforte maker, certainly thought Prof. Bryan's invention gave a greater control over the instrument to the gifted musician. With regard to the question of varying the time of contact of hammer with string by variations in the touch, in his opinion it was impossible to do this. When the hammer was in contact with the string the key was entirely disconnected from it, and also the *check* was merely a repetition device, and did not come into action until the hammer had left the string. The only thing that could be varied (neglecting the loud and soft pedals) was the velocity with which the hammer struck. It seemed to him that with Prof. Bryan's invention a greater control was obtained over the velocity. Kaufmann's investigation, to which Prof. Bryan referred in his Paper, neglects a point of primary importance in the pianoforte. Stokes had shown that a vibrating string, if insulated from a sound board or resonator, was inaudible ; therefore the sound from a pianoforte was produced, not by the string, but by the sound board. The latter had a natural frequency of its own, and was set in vibration by the blow of the hammer transmitted through the strings. The natural frequency of the sound board was rapidly damped. (Some experiments of his own showed that it seldom gave more than about six waves, the first two or three being of larger amplitude than the forced vibrations caused by the string.) Was it not highly probable that the varying strengths of the harmonics set up by different velocities of the striking hammer were caused by the difference in the amount of energy absorbed in producing the natural frequency of the sound board ?

Mr. V. Lough (communicated remarks) considered that as an addition

to the ordinary pneumatic player Prof. Bryan's expression regulator should prove very useful. It certainly enabled a skilled operator to make a great improvement in the rendering of the music. There were, of course, other arrangements by which the pressure in these players could be varied suddenly to emphasise particular notes or chords; but Prof. Bryan was, he thought, the first to introduce the principle of variable inertia in the pressure regulator. Although the conditions of the performance were not very favourable, there was certainly a very marked difference between the "metallic" and the "richer" effects, and this he supposed meant a change of tone quality; but how much of this was due to the regulator was not clear, owing to the use of the sustaining pedal. He could, however, detect little in favour of Prof. Bryan's claim of differentiating between notes struck simultaneously, an effect which was obtainable in some players. In any case there were none of the clear singing tones and delicate touch effects which could be produced by a good pianist, but were still beyond the range of the mechanical player. As regarded the scientific aspect of the matter, the Paper did very little to justify its title. Very little information was given as to the analysis of the dynamics of the hammer stroke, and the results were only vaguely indicated. The problem of the variation of tone quality with touch was not appreciably advanced by this Paper.

Mr. T. Harding Churton (communicated remarks) stated that Prof. Bryan referred to what he described as the missing element of "touch" in the performance of the mechanical piano-player, and discussed what was meant by "touch." An examination of the mechanism of a piano made it evident that, no matter how a key was depressed, the result depended entirely upon the velocity imparted to the hammer that struck the wire or wires, and that the only further effect of holding the key down was to hold the damper off the wires, and thus allow the vibration to die down more slowly than if the damper were allowed to resume contact with the wires earlier. It was, therefore, evident that no variety of tone could be produced in a piano that depended upon the striking of the key which could not be produced by suitable mechanical means for striking the key with the required degree of force. But what was ordinarily conveyed by the term "touch" was not the tone produced, nor merely the degree of force used, but the effect produced by (1) the *relative* loudness of the notes played, whether struck simultaneously or in sequence; (2) the relative duration of the notes and intervals of rest ("staccato" or "legato"); and (3) by slight variations from the strict time of playing individual notes or group of notes. In short, the effects of "touch" depended upon the particular manner in which notes were played with respect to force and time, and the value of these were generally being continually varied in a more or less complex manner by a skilful player playing by hand. The more regular or uniform performance of the mechanical player constitutes, in fact, the characteristic difference between mechanical and hand playing. The music roll was capable of being cut so as to reproduce the playing of a pianist as regards *time*, but until the mechanical player was also provided with automatic control of the *force* with which each individual note was struck, the effect of "touch" must remain its missing element.

The Author, in reply, wrote: The fundamental question which was raised by my Paper may perhaps be best stated as follows. When the key of a pianoforte is depressed, either by the finger or by pneumatic action, is the resulting effect a function of one variable only (namely, the striking velocity), or is it a function of other variables as well? The assumption is, of course, made that certain other conditions remain the same in all cases. For example, the sustaining pedal must either be held down in every case, or, if this is not done, the note must always be held down for the same length of time after it has been struck. In adopting the latter method the experiments can be more easily made with the pneumatic player, since it is not easy to strike a key sharply with a finger and at the same time hold it down suffi-

ciently to keep the damper from descending. Now I entirely agree with Mr. G. H. Berry that the construction of a pianoforte renders it very difficult to accept anything but the one-variable theory; at the same time I am bound to state that I find it impossible to disbelieve in the two (or more) variable hypothesis, in view of results which I have been able to obtain on a piano, with a piano-player in a room, with all of which I have become familiar after 10 years' experience. The discussion at the Physical Society afforded some opportunity for an inquiry into this paradoxical question; at the same time I have made numerous inquiries in other directions, and I find that there is a widespread belief in what I have called the two-variable theory. Further, I am told that in Germany, in teaching the pianoforte, this theory is tacitly assumed, two different kinds of touch being distinguished as apart from the mere differences of loudness or softness. On the other hand, it is a natural corollary of the "one-variable" theory that when the sustaining pedal is held down it makes no difference whether notes are played "staccato" or "legato," yet a great many pianists are very careful to maintain this (under such circumstances unnecessary) distinction. Some important light has been thrown on this matter by Mr. W. H. Gray, B.Sc., of Bangor, a research student in chemistry who is also a first-class pianist, and with whom I recently had a conversation on the subject. Unfortunately, he was unable to give me any references to the published Papers on the subject which he had read. His conclusions were, however, somewhat as follows: (1) It is possible to vary the quality of a pianoforte note, independent of its loudness or softness, to an extent which has a very appreciable influence on the musical effect. (2) A pressure strong at first and rapidly decreasing as the key descends produces a brilliant tone rich in harmonics. (3) A pressure gradually increasing as the key descends produces a softer tone in which the harmonics are subdued. (4) *It has been proved experimentally* that these differences are associated with differences in the length of time that the hammer remains in contact with the string. As in Kaufmann's Paper, the striking part of the hammer was covered with metallic foil, and the time of contact determined by carrying an electric current through the wire and hammer. (5) *These differences are due to vibrations set up in the rod of the pianoforte hammer.* Conclusions 1, 2, 3, are identical with the results which I had obtained independently by means of the piano-player, and which were unknown to Mr. Gray at the time he stated them. With regard to (4), it is desirable to obtain further information about these experiments. The explanation (5) seems rather improbable at first sight, but there are several points in its favour. The force applied to raise the hammer is, owing to the short leverage, large compared with its weight, and the bending moment set up near the end is considerable. The vibrations, doubtless, die out rapidly, but the interval between release and striking is very small indeed. Similar vibrations can be easily visualised on a large scale in a billiard cue. Mr. Berry refers to one vibrating system that has not been taken into account—namely, the sounding board. I think under the circumstances that the vibrations set up in the hammer may be equally important. An object lesson can be obtained by experimenting with a wine glass, in which the first harmonic is usually stronger than the fundamental, but by suitably striking the glass either may be made to predominate at will. Mr. Lough seems to have overlooked the fact that the Paper was only the first part of a more general investigation of the dynamics of the pianoforte hammer, and, in view of the evidence now elicited it is most fortunate that the second part, to include the actual equations of motion, was not published, as it would certainly not have taken account of some of the conditions which are now shown to be necessary. I now propose to defer the matter still further, pending inquiries as to the experiments referred to by Mr. Gray. With regard to the question of obtaining delicate *pianissimos* with a piano-player, as referred to by Dr. Eccles, the illustrations were evidently very unsatisfactory in this respect, owing to a mistake in estimating the acoustic pro-

perties of the room. In order to obtain the highest possible effects it is of course necessary (1) that the piano and player should be in perfect adjustment; (2) that the springs of the bellows should be of the right strength, which is usually far from being the case, and here the auxiliary lever comes in useful; (3) that the performer should be thoroughly accustomed to his instruments; (4) that the piano should be kept open, otherwise the higher harmonics are absorbed; (5) that the performer should concentrate his thoughts on the music, performing the necessary manipulations instinctively and not consciously; (6) that he should be in good form at the time, this being perhaps the most important item of all. In comparing pneumatic with finger playing in this matter I consider that the possibilities are certainly not unfavourable to the pneumatic player. It appears by no means easy for an ordinary pianist to obtain the most delicate effects, and very few of the professional and other performers at the concerts of our local musical club obtain such good results as are certainly *obtainable* with a piano-player. As regards the few exceptional pianists who obtain such marvellous *pianissimos*, it would be necessary, in order to institute a satisfactory comparison with their results, that a performer should have given as much time and attention to practising the piano-player that they have given to practising the piano. An amateur using his piano-player only in leisure half hours cannot expect to get anywhere near Paderewski. It would be undesirable, however, to digress much further on the subject of piano-players apart from the dynamical and acoustical properties suggested by them. But, judging from Mr. Campbell's remarks, I think it is rather a pity that he does not use a piano-player, as he would find that great charm and interest is afforded by these attempts to analyse pianoforte touch, and to utilise the results in the interpretation of classical music.

[It has since been proved that the absence of delicate effects in the illustrations shown was entirely due to the stretching of the copper wire used in connecting the lever with the bellows of the player. When a similar wire was subsequently tried in my piano-player at Bangor the playing sounded exactly like that shown at the Imperial College, there being a similar slight harshness of tone and insufficient differentiation between different parts of chords. On substituting a less extensible covered steel picture-wire the softer effects again became obtainable.—*April* 24, 1913. G. H. B.]

# XV. *A Graphic Method of Optical Imagery.* *By* William R. Bower, *B.Sc., A.R.C.S., Technical College, Huddersfield.*

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CONTENTS.



1. The object of this Paper is to show how the important properties of an optical system may be readily deduced from diagrams drawn on the following simple principle: The vertex (called the object or radiant) of a small incident pencil of rays is considered to move along a line parallel to a principal axis of the system. This line is also regarded as the direction of the incident portion of a fixed ray. Then the directions of the ray after refraction or reflection at the several surfaces do not alter as the position of the object changes. These directions are therefore the fixed loci of the images or vertices of the refracted and reflected pencils. By the diagrams obtained, the path of any ray right through the system may be traced and the successive positions of the images resulting from a progressive movement of the object exhibited. Also the constructions are such that, in the case of a thick lens, the importance of the cardinal points and the planes associated with them is demonstrated. I ask especially for the criticism of teachers, to them and their less advanced pupils the method appeals, because it visualises what is usually accomplished

by somewhat mechanical algebraic treatment. Graphic methods have previously been given by which the incident and emergent portions of a ray through an optical system can be identified. Such methods may be convenient to adopt after a time, but the beginner is like a young traveller who wants to know not only where to start and where to finish, but also something of what happens on the way.

*Rule of Signs.*—The one adopted is optically old-fashioned. The + sense of a line is the reverse to that of the incident light: if this travels from right to left, then the + sense of a line is from left to right. Also the + sense of vertical lines is from below upwards.

## I. Spherical Surface.

2. *Refraction at a Spherical Surface* (Fig. 1).—Let AB represent the spherical refracting surface, centre C, radius $r$, sepa-

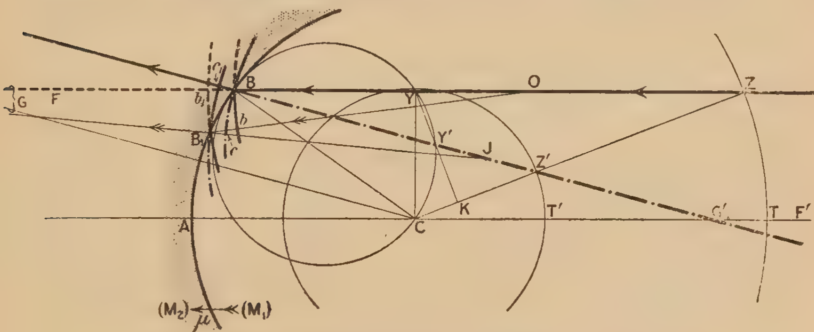


Fig. 1.—Refraction at a Spherical Surface.

rating the media $M_1$, $M_2$, for which the index of refraction is $\mu$. Assume $\mu$=(velocity of light in $M_1$)/(velocity of light in $M_2$). Through any point O, and each of any two points B, $B_1$, on the circle AB, draw BO, $B_1O$. Draw the circular arcs $Bb$, $B_1b_1$, having their centre at O, and of radii BO, $B_1O$, respectively.

Suppose $Bb$, $B_1b_1$ represent two positions of a portion of a spherical wave. This undergoes refraction between B, $B_1$. Let $Bc$, $B_1c_1$ be the corresponding positions of the refracted wave, and $c_1BJ$ be the normal at $c_1$ to the refracted wave. Then $Bb_1=\mu \,.\, Bc_1$.

On BC as diameter describe a circle cutting BO in Y and BJ in Y′. Draw YY′, also CK perpendicular to YY′. Produce CK to cut BO in Z and BJ in Z′.

When B, $B_1$ are very close, the arcs $BB_1$, $Bb$, $Bc$ are sensibly straight and perpendicular to the respective radii BC, BO, BJ. Then the triangles $Bc_1b_1$ and CY′Y are similar. $\therefore$ CY$=\mu$. CY′. Then Y′ is a fixed point when the line BY is fixed relatively to AB.

3. It is now convenient to deal with rays instead of waves. Let $\varphi$, $\varphi'$ be the angles of incidence and refraction respectively.

(i.) Since $\mu$=CY/CY′, $\therefore$ sin $\varphi$/sin $\varphi'$=sin CY′Y/sin CYY′ $=\mu$. Thus the second law of refraction is deduced.

(ii.) The angles CZ′B, CY′K are each equal to $\varphi$, and CZB, CYK to $\varphi'$.

Then, by similar triangles, BZ/BZ′=CY/CY′$=\mu$.

Also BZ/CZ=BZ′/BC ; $\therefore$ CZ$=\mu r$.

Also CZ′/BZ′=BC/BZ ; $\therefore$ CZ′$=r/\mu$.

Thus the locus of Z for any incident ray is the circle of radius $\mu r$ and centre C. And that of Z′ is the concentric circle of radius $r/\mu$.

Z, Z′ are the *aplanatic pair of points* for an incident pencil diverging from Z, whose chief ray is along CZ.

Draw AC parallel to the incident ray, cutting BZ′ at G′ and the circular loci of Z, Z′ at T, T′. Also draw GC parallel to BG′, cutting OB in G. Then BG′/CG′=CY/CY′$=\mu$. And GB=CG′=BG′$/\mu$.

(iii.) As the angle of incidence CBZ diminishes, Z, Z′ move along their circular loci towards T, T′ respectively ; K also approaches C, its locus being the circle on CY as diameter.

## 4. *Refraction at a Spherical Surface : Normal Incidence.*

(iv.) At normal incidence, that is, when $\varphi$ is very small, Y′ sensibly lies on CY and K coincides with C. Also Z and Z′ are close to T, T′, respectively ; and, sensibly, BZ=AT$=(\mu+1)r$ ; BZ =AT′$=(\mu+1)r/\mu$.

Suppose F and F′ to be the respective positions of G, G′ when $\varphi$ is 0. Then AF′=BF′$=\mu$. CF′$=\mu$(AF′—AC) $=\mu r/(\mu-1)$. And BF=F′C=—(AF′—AC)$=-r/(\mu-1)$.

F, F′ are respectively the first and second principal foci of a pencil incident normally at A.

(v.) When the incidence is normal it is convenient to increase largely the scale of length perpendicular to the principal axis in comparison with that along the axis. This is legitimate in an elementary discussion restricted to formulæ

of the first order, provided that trigonometric relations are used cautiously. The difference of scale gives a distorted figure in which angles that are actually equal appear unequal. But the relative lengths of lines perpendicular to the axis will be maintained, and also that of lengths parallel to the principal axis.

(vi.) In Fig. 2, let AB represent the spherical surface and C its centre.

Draw any incident ray, BO, parallel to the principal axis, AC. Draw CY perpendicular to BO and cut off $CY' = CY/\mu$. Draw the refracted ray BY′, cutting AC in F′.

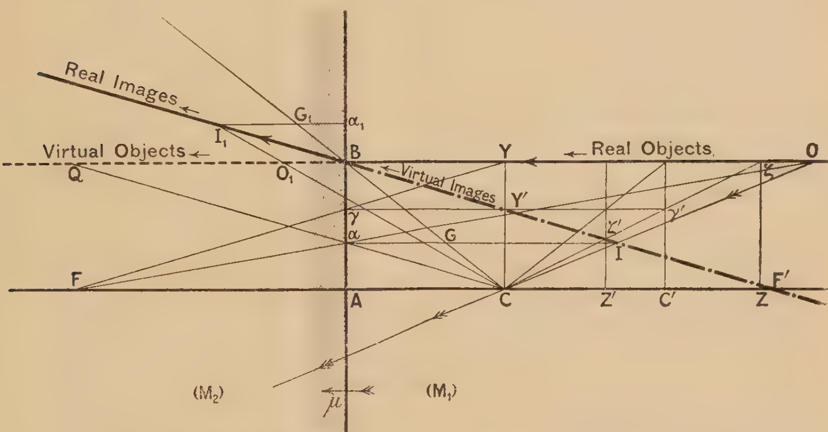


FIG. 2.—REFRACTION AT A SPHERICAL SURFACE. NORMAL INCIDENCE.

When the object is in some position O, draw CO, cutting the refracted ray at I. Then I is the image of O.

When O is at infinity, I coincides with F′. Thus, F′ is the *second principal focus*.

When O is at a point Q, such that QC is parallel to the refracted ray BF′, then the image is at infinity. Draw FQ parallel to AB, cutting AC in F. Then F is the *first principal focus*.

Then $CF' = FA$; $FC = AF'$; $AF' = AC + FA$.

Also $AF'/FA = FC/FA = CY/CY' = \mu$.

Also $FA/AC = A\gamma/Y'Y = 1/(\mu - 1)$.

Also $AF'/AC = AB/\gamma B = \mu/(\mu - 1)$.

Through I draw $\alpha$I parallel to AC, cutting AB in $\alpha$. Then O$\alpha$F are collinear. $\therefore$ QO/BO=AB/$\alpha$B=AF$'$/$\alpha$I.

$\therefore$ (FA+BO)/BO=AF$'$/$\alpha$I.

5. It is convenient to express some of these results algebraically. Write $r$ for the radius, $u$, $v$, $f$, $f'$ for the respective distances of the object, image, first and second principal foci from the refracting surface, and regard them as + when measured in the opposite sense to that of the light, and − when in the same sense. Then

(i.) $f'+\mu\,.\,f=0.$ (ii.) $f+f'=r.$

(iii.) $f=-r/(\mu-1).$ (iv.) $f'=r\,.\,\mu/(\mu-1).$

(v.) $\dfrac{f}{u}+\dfrac{f'}{v}=1.$ (vi.) $\dfrac{\mu}{v}-\dfrac{1}{u}=\dfrac{\mu-1}{r}.$

6. *Construction for the Principal Foci*, F, F$'$, *when $r$ and $\mu$ are given.*—Draw, as above, AC=$r$; AB; BO at any distance; CY. Make CY$'$=CY/$\mu$. Draw BY$'$F$'$; then $\gamma$Y$'$ parallel to AC, cutting AB in $\gamma$; then Y$\gamma$, cutting AC in F.

*Further construction for the Aplanatic Points*, Z, Z$'$.—Make CC$'$=$r$. Draw C$'\gamma'$ perpendicular to CC$'$, meeting $\gamma$Y$'$ in $\gamma'$. Draw C$\gamma'$, cutting BO in $\zeta$ and BF$'$ in $\zeta'$. Draw $\zeta$Z and $\zeta'$Z$'$ perpendicular to AF$'$ and meeting it in Z, Z$'$.

## II. Thick Lens.

7. *Thick Lens.*—In Fig. 3, suppose two spherical surfaces to separate media $M_1$, $M_2$, $M_3$. Draw the principal axis through the respective centres $C_1$, $C_2$, and cutting the surfaces at $A_1$, $A_2$. Mark off $F_1$, $F_1'$, the first and second principal foci of the surface through $A_1$, and similarly $F_2$, $F_2'$ for the other surface.

Take any position, O, of the luminous point.

Draw $OB_1$ parallel to the principal axis. This represents the incident ray from O that is parallel to the principal axis. $B_1F_1'$ is the corresponding refracted ray; this is incident upon the second refracting surface at $B_2$.

Draw $C_1O$. The point L where it cuts $B_1F_1'$ is the image of O formed by the first surface. Draw $F_2L$ cutting the surfaces in $P_1$, $P_2$ respectively. Draw $P_2I$ parallel to the principal axis, also $C_2LI$ meeting $P_2I$ at I. Then I is the final image formed by the system.

Draw $B_2I$ crossing $A_1A_2$ at F$'$. This line is the direction of $B_2B_1$ after refraction at the second surface. Then the lines

$B_1O$, $B_1B_2$, $B_2I$ are the directions of what may be called the *second fixed ray* of the system, since their positions are unaltered, and also that of $B'$ (the intersection of $B_2F'$ and $B_1O$) for every point occupied by the object along $B_1O$.

Draw $P_1O$ crossing $A_1A_2$ at $F$; produce to meet $P_2I$ at $P$. Through $F'$, $B'$, $P$ and $F$ draw parallels to $A_1B_1$, and mark off the obvious crossing points $P'$, $A'$, $A$, $B$ and $Q$.

8. In dealing with the loci of the images formed by the surfaces as the position of the object is systematically altered,

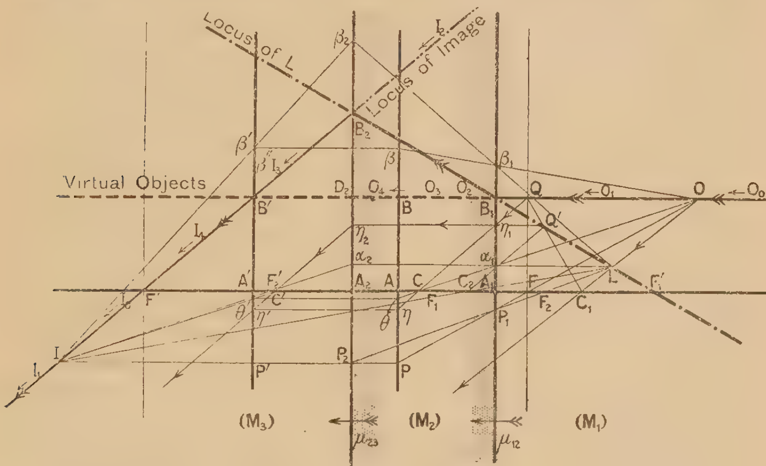


FIG. 3.—THICK LENS.

it is convenient to consider a radiant O, that is the vertex of a normally incident pencil, to pass along $OB_1$ from the extreme right to the extreme left. To show that—

(i.) *$F$ and $F'$ are fixed points.* All rays that emerge parallel to $A_1A_2$ diverge from $F_2$. Now, by construction, one ray $P_1F$ through $F$, after refraction at the first surface passes through $F_2$; therefore, since the incidence is normal, all rays through $F$ in medium $M_1$ are refracted so that in $M_2$ they diverge from $F_2$. $F$ and $F_2$ are therefore in regard to the first surface related as object and image. Similarly, $F_1'$ and $F'$ in regard to the second surface. Then $F$ and $F'$ are fixed because $F_2$ and $F_1'$ are fixed. Thus, for each position of the object the ray corresponding to $P_1O$ passes through $F$.

(ii.) *The locus of $P$ is parallel to $A_1B_1$.*

$$\frac{AF}{A_1F}=\frac{AP}{A_1P_1}=\frac{A_2P_2}{A_1P_1}=\frac{A_2F_2}{A_1F_2}. \quad \therefore AF=\frac{A_2F_2}{A_1F_2}\times A_1F,$$

and is constant. Then, as F is fixed for all incident rays, the locus of P is the line (and plane) through A parallel to $A_1B_1$.

(iii.) *The locus of B′ is parallel to $A_2B_2$.* Suppose a second incident ray $ob_1$, parallel to $OB_1$ (not drawn in the figure: the points corresponding to those on the diagram are indicated by small letters of the same name). Also the subsequent directions, $F_1'b_1b_2$ and $b_2b'F'$. Then

$$\frac{d_2b'}{D_2B'}=\frac{d_2b'}{A_2F'}\times\frac{A_2F'}{D_2B'}=\frac{d_2b_2}{A_2b_2}\times\frac{A_2B_2}{D_2B_2}=\frac{A_2A_1}{A_2F_1'}\times\frac{A_2F_1'}{A_2A_1}=1.$$

$\therefore$ $d_2b'=D_2B'$, and hence the locus of B′ is the straight line (and plane) through A′ parallel to $A_2B_2$.

The locus of P is the first principal or unit plane, that of B′ the second; A, A′ are the first and second principal points, F, F′ the first and second principal foci.

(iv.) As O approaches Q from the extreme right, the slope of OFP increases, and P and I (whose locus is $B_2I$) get further from $A_1A_2$.

When the radiant is at Q, QF is parallel to $B_1A_1$: then P and the image are at infinity and the emergent rays, and therefore $C_2Q'$ (Q′ is the intersection of $C_1Q$ and $B_1F_1'$) are parallel to B′F′.

As the radiant leaves Q, the image passes along and in the direction B′F′, towards F′. Also P moves downwards along BA towards A.

When the image is at B′ so is P′; at the same time the radiant and P are at B.

9. *The Principal Planes are planes of unit magnification.*—(i.) B′ is the image of B and BB′ is parallel to AA′. Also, if another ray (not shown in the figure) parallel to the principal axis passes through the first and second principal planes at $b$, $b'$ respectively, then $b'$ will be the image of $b$. Hence an object, B$b$, will have an equal image, B′$b'$.

(ii.) To prove that any incident ray, $O\beta_1$, and its emergent portion, $\beta_2I$, intersect the first and second principal planes respectively at points, $\beta$, $\beta'$, that are equidistant from the principal axis.

Since the principal planes are planes of unit magnification, an object at $\beta$ on the first principal plane will have an image at, say, $\beta''$ on the second principal plane, and $A'\beta''=A\beta$. Now $O\beta_1\beta$ could be a ray incident at $\beta_1$, hence the emergent portion passes through $\beta''$. But it passes through $\beta'$. Hence $\beta'$ and $\beta''$ are coincident.

*Alternative proof—*

$$\left.\begin{aligned}\frac{\beta P}{\beta_1 P_1}&=\frac{BO}{B_1O}=\frac{BP}{B_1P_1}\\ \frac{\beta_1P_1}{\beta_2P_2}&=\frac{La_1}{La_2}=\frac{B_1P_1}{B_2P_2}\\ \frac{\beta_2P_2}{\beta'P'}&=\frac{P_2I}{P'I}=\frac{B_2P_2}{B'P'}\end{aligned}\right\}\quad \begin{aligned}&\text{Multiply the columns together.}\\ &\therefore\ \frac{\beta P}{\beta' P'}=\frac{BP}{B'P'}=1.\\ &\therefore\ \beta P=\beta' P'.\\ &\therefore\ \beta' A'=\beta A.\end{aligned}$$

10. *The Nodal Points.*—(Fig. 3.)—Suppose the object to be at Q in the first focal plane. Draw an incident ray, QC, parallel to the fixed emergent ray, $B_2F'$, cutting $A_1A_2$ in C, also $A_1B_1$ in $\eta_1$ and AB in $\eta$. Draw $QC_1$ cutting $B_1F_1'$ in Q′. Draw $Q'\eta_1$ cutting $A_2B_2$ in $\eta_2$. Draw $\eta_2C'$ parallel to $B_2F'$, cutting $A_1A_2$ in C′, also A′B′ in $\eta'$.

$Q\eta_1$, $\eta_1\eta_2$, and $\eta_2\eta'$ are the three portions of a ray through the system. C, C′ are the *first* and *second nodal points*. Also C and C′ are related as object and image points.

11. *Properties of the Nodal Points.*—(i.) The distance between the nodal points is equal to the distance between the principal points.

Since $A'\eta'=A\eta$, the similar triangles $AC\eta$, $A'C'\eta'$ are equal. Then A′C′=AC and ∴ CC′=AA′.

(ii.) When the incident portion of a ray passes through the first nodal point, its emergent portion passes through the second, and is parallel to the incident portion.

Take any position, O, of the radiant. Draw OC cutting AB in $\theta$. Draw $\theta\theta'$ parallel to $A_1A_2$. The emergent ray goes through $\theta'$. It also goes through C′, because C′ is the image of C. It also passes through I. Thus I, $\theta'$ and C′ are collinear. Then, since A′C′=AC and $A'\theta'=A\theta$, the triangles $A'C'\theta'$ and $AC\theta$ are equal. ∴ C′I is parallel to CO.

12. *Construction for the Six Cardinal Points and Planes* (Fig. 4).—Draw the principal axis of the system and mark off $A_1$, $A_2$, $C_1$, $C_2$, $F_1'$, $F_2$. Draw any parallel to the principal axis $OB_1$, cutting the surface through $A_1$ in $B_1$, and the surface through $A_2$ in $D_2$. Draw lines through $B_1$, $F_1'$ and $D_2$, $F_2$ intersecting at S. Draw lines through S, $C_1$ and S, $C_2$ intersecting $OB_1$ at B, B′ respectively. Then B′ is the final image of B, and the object, AB, has an equal image, A′B′. Thus, B, B′ *are points on the first and second principal planes respectively.*

Let $D_1$ be the point of intersection of $A_1B_1$ and $D_2F_2$. Join $BD_1$ and produce to cut $A_2A_1$ in F. Also draw $B_2B'$, cutting $A_1A_2$ in F′. Then F, F′ *are the first and second principal focal points.*

Draw FQ parallel to $A_1B_1$, meeting $B_1D_2$ in Q. Draw QC parallel to $B_2F'$. Make CC′ equal to AA′. Then C, C′ *are the first and second nodal points.*

Complete the three directions through $B_1B$, $B_1B_2$, $B_2B'$, o the second fixed ray. For any position, O, of the object on the incident direction through $B_1B$, the first image, L, is on the intermediate direction through $B_1$ $B_2$, and is found at the intersection, L, of $B_1B_2$ with $OC_1$. The final image, I, is on the

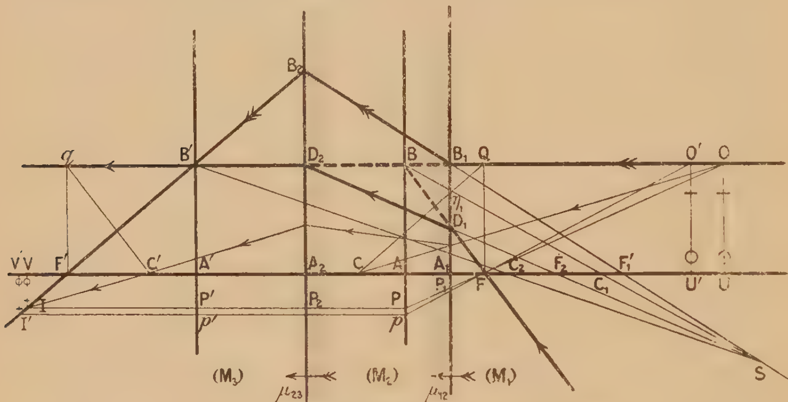


FIG. 4.—CARDINAL POINTS OF A THICK LENS.

emergent direction through $B_2B'$, and is at the point where $LC_2$ cuts $B_2B'$. The final images are real when I is on the actual emergent ray, and virtual when on its backward continuation.

Complete the three directions $D_2B'$, $D_2D_1$, $BD_1$ of what may be called the *first fixed ray* through the system. The positions of images for any position of the object is found as in the previous case.

If the direction of the light is reversed, so that it is first in the medium, $M_3$, and incident on $A_2B_2$, then the positions of the cardinal points are unaltered. The names, however, must be interchanged, a first point in the former case becoming a second in this, and conversely. To demonstrate these matters in regard to the principal and focal points, consider a point object to move along B′B from the extreme left. To show that

the nodal points are unaltered draw $F'q$ parallel to $A_2D_2$, meeting $D_2B'$ in $q$. Draw $qC'$. Then $F'C'=F'A'-C'A'$ $=CF-CA=AF$. $\therefore qC'$ and BF are parallel.

13. (i.) *Relations between the Focal Lengths*, A′F′ and AF (Fig. 4).—

$$\frac{AF}{BB_1}=\frac{AB}{D_1B_1}=\frac{A_2F_2}{A_2A_1}; \quad \frac{F'A'}{B'D_2}=\frac{A'B'}{D_2B_2}=\frac{A_1F_1'}{A_2A_1}; \quad \frac{BB_1}{B'D_2}=\frac{C_1F_1'}{C_2F_2}.$$

$$\therefore \frac{AF}{F'A'}=\frac{C_1F_1'}{C_2F_2}\cdot\frac{A_2F_2}{A_1F_1'}=\frac{1}{\mu_{12}\cdot\mu_{23}}=\mu_{31}.$$

$$\therefore A'F'+\mu_{13}\,.\,AF=0.$$

(ii.) *Distances of the Nodal Points*, AC and A′C′.—

$$AC=FC-FA=A'F'+AF=f+f'.$$

$$A'C'=A'F'-C'F'=A'F'+AF=f+f'.$$

(iii.) When the media $M_1$, $M_3$ are the same, $\mu_{13}=1$, then $f'=-f$, and the nodal points C, C′ coincide with the principal points A, A′.

(iv.) *Standard Definitions of the Focal Lengths.*—

$$\tan FBQ=F'q/AF, \qquad \tan F'B'q=F'A'/FQ.$$

$\therefore f=F'q/\tan FBQ$, or the first principal focal length is the ratio of the length of the image formed in the second focal plane to the apparent or angular magnitude of the corresponding infinitely distant object.

Also $f'=FQ/\tan F'B'q$, or the second principal focal length is the ratio of the length of the object when in the first focal plane to the apparent or angular magnitude of the corresponding infinitely distant image.

These are the Gauss and Abbe definitions of the focal lengths of an optical system.

14. *Focal Distances, &c.*—Suppose an object, OU, and the corresponding image, VI, obtained by drawing OFP; then PP′I parallel to AA′, and meeting B′F′ in I. Then $VF'/F'A'$ $=PA/AB=AF/FU$.

Write $AU=u$, $A'V=v$, $AF=f$, $A'F'=f'$.

$$\therefore \frac{v-f'}{f'}=\frac{f}{u-f}. \qquad \therefore \frac{f}{u}+\frac{f'}{v}=1.$$

Also $f'/f=-\mu_{13}$. $\therefore \mu_{13}/v-1/u=-1/f=\mu_{13}/f'$.

15. *Magnification.*—(i.) *Lateral Magnification* ($m$) or (length of image)/(length of object).

$$m=-\mathrm{IV}/\mathrm{F}'q=-\mathrm{VF}'/\mathrm{F}'\mathrm{A}'=(f'-v)/f'=f/(f-u).$$

(ii.) *Axial or Depth Magnification* ($d$) or (axial displacement of the image)/(very small axial displacement of the object producing it).—Let I′V′ be the position of the image when the object is at U′O′. Then VV′/OO′=VF′/QO′.

When O′ is very close to O, VV′/OO′ is the depth magnification. Also QO′=QO sensibly.

$$\therefore\ d=-(f'-v)/(f-u)=-ff'/(f-u)^2=\mu_{13}\cdot m^2.$$

(iii.) *Angular Magnification or Convergence-ratio* ($w$) or (angle between two emergent rays through an image point) ÷(angle between the two corresponding incident rays).

Then

$$w=\frac{\text{angle B}'\text{IP}'}{\text{angle POB}}=\frac{\text{P}'\text{B}'}{\text{IP}'}\bigg/\frac{\text{PB}}{\text{BO}}=\frac{\text{BO}}{\text{IP}'}=-\frac{u}{v}=\frac{f-u}{f'}=\frac{f}{f'-v},$$

or $$w=\frac{\text{angle C}'\text{IF}'}{\text{angle COB}}=\frac{\text{angle C}'\text{IF}'}{\text{angle IC}'\text{F}'}=\frac{\text{C}'\text{F}'}{\text{IF}'}=\frac{-\text{AF}}{\text{VF}'}=\frac{f}{f'-v}.$$

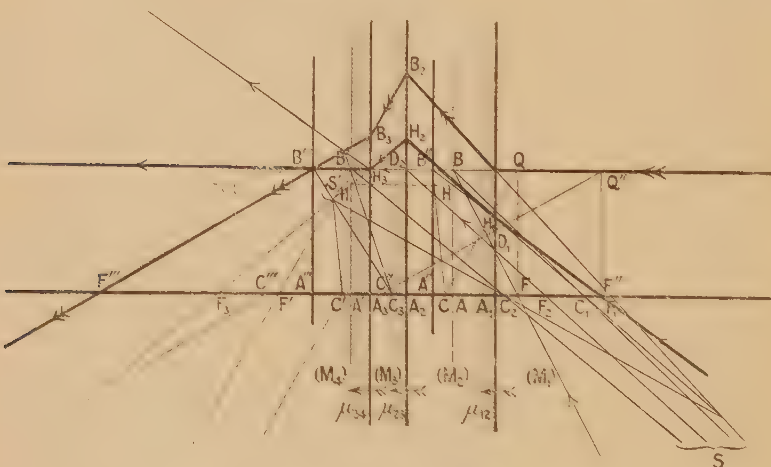


FIG. 5.—COMPOSITION OF CARDINAL POINTS.

16. *Extension to Three Coaxial Refracting Surfaces* (Fig. 5).—Suppose a third surface through $A_3$, centre $C_3$, first principal focus $F_3$. For the surfaces through $A_1$, $A_2$ find B, B′, F, F′, C, C′.

Let QB pierce the surface $A_3$ at $H_3$, and $H_3$ $F_3$ intersect $B'F'$ at $S'$. Draw $S'C_3$ to cut QB in $B'''$. Then $B'''$ is on the *second principal plane* of the system. Let $B'''$ $B_3$ intersect the principal axis at $F'''$. Then $F'''$ is the *second principal focus* of the system.

Draw $CB''$ parallel to $S'C'$, intersecting QB in $B''$; then $B''$ is a point on the first principal plane of the system.

Let $H_3F_3$ pierce $A'B'$ at $H'$. Make $HA=H'A'$. Then $F''$, where $B''H$ cuts the principal axis, is the *first principal focus* of the system.

From $Q''$, where QB pierces the focal plane through $F''$, draw $Q''C''$ parallel to $F'''B_3$ and cutting the principal axis at $C''$. Make $C'''C''=A'''A''$. Then $C''$, $C'''$ are the *first and second nodal points* of the system.

Thus the six cardinal points of a system are compounded with those of a refracting surface, and the whole reduced to six cardinal points. These may again be compounded with another refracting system and so on.

## III. Spherical Surface (*continued*).

17. (i.) *Young's construction for the Refracted Ray* (Fig. 6).—From C, the centre of the spherical surface (radius $r$), describe circles of radii $r\mu$ and $r/\mu$. Let the incident ray, BO, cut the former circle at Z; join CZ cutting the latter circle at Z′. Then the direction of the refracted ray is through BZ′. Z, Z′ are the aplanatic pair of points for the ray ZB, Z′B. (*See* § 3.)

(ii.) *Alternative construction.*—On BC as diameter draw a circle cutting BO in Y. Draw the chord $CY'=CY/\mu$. Then the refracted ray passes through B, Y′.

18. *Meridian Rays.*—(iii.) YY′ crosses CZ at right angles at the point K (called, by Cornu, the *junction point*).

(iv.) Let $Z$, $Z'$, $Z_1$, $Z_1'$ be the aplanatic pairs of points for two parallel rays incident at B, $B_1$, respectively. Let the corresponding refracted rays intersect at E′.

Through $Z_1'$ draw a parallel to BZ′, cutting the chord $B_1B$ at $b_2$. Draw $Z_1b_2e$, cutting ZB in $e$. Now,

$$\frac{eB}{B_1Z_1}=\frac{b_2B}{B_1b_2}=\frac{Z_1'E'}{B_1Z_1'} \qquad \therefore \frac{eB}{Z_1'E'}=\frac{B_1Z_1}{B_1Z_1'}=\mu.]$$

(v.) Draw $CY_1Y$ perpendicular to the parallels $B_1Z_1$, BZ; also YK, $Y_1K_1$ perpendicular to CZ, $CZ_1$ respectively. Through

K draw KF′, KF parallels to BZ, BZ′ respectively; similarly $K_1F_1'$. Then $FB = KF' = \mu \cdot Z'F'$.

(vi.) When $B_1$ is very close to B, $Y_1$, whose locus is CY, is very close to Y and $K_1$ to K. Then sensibly $F_1'$, and E′ coincide with F′. Also $e$ coincides with F, for in all cases $eB = \mu \cdot Z_1'E'$.

Also the chord $BB_1$ very closely coincides with the arc $BB_1$, and the geometrical relationships deduced from the finite chord become optical relationships for rays of light refracted at the infinitesimal arc. The above relations are therefore approximately true for a small arc.

(vii.) Suppose $OO_1$ is a position of a very small object moving between BZ, $B_3Z_3$. Join $FO_1$ cutting the chord and

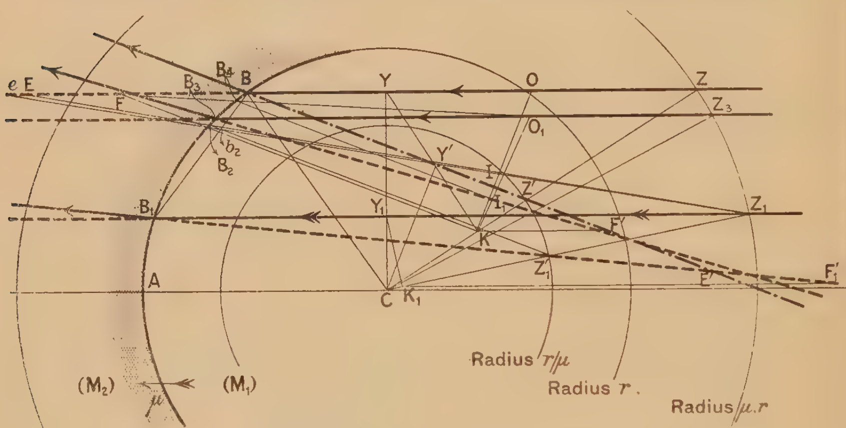


FIG. 6.—REFRACTION AT A SPHERICAL SURFACE. MERIDIAN RAYS.

arc, $B_3B$, at $B_4$. Draw $B_4I_1$ parallel to BF′, cutting $B_3F'$ at $I_1$. Then $I_1$ is the image of $O_1$.

Join $I_1O_1$, also $KI_1$. Then

$$\frac{B_3O_1}{FB} = \frac{B_3B_4}{B_4B} = \frac{B_3I_1}{I_1F'} \qquad \therefore \frac{B_3O_1}{B_3I_1} = \frac{KF'}{I_1F'}$$

Then, since KF′ is parallel to $B_3O_1$, the points $O_1$, $I_1$, K are collinear.

When the object is very small, $O_1$ coincides with O, and $I_1$ with I. Hence the position of the image on the refracted ray BZ′, corresponding to any position O of the object on the incident ray, BZ, is at the point I where the line through OK cuts the refracted ray BZ′.

Then, considering the object to move from the extreme right

to the extreme left, F, F′ are identified as the first and second *principal focal points* for narrow meridional pencils having BZ for their chief ray.

(viii.) Since (Fig. 7)

$$\frac{FB}{BO}=\frac{KF'}{BO}=\frac{IF'}{BI}=\frac{BF'-BI}{BI}, \quad \therefore \frac{BF}{BO}+\frac{BF'}{BI}=1.$$

(ix.) $$\frac{KF'}{BY}=\frac{KY'}{Y'Y'} \quad \frac{FK}{BY'}=\frac{KY}{Y'Y'} \quad \therefore \frac{KF'}{FK}=\frac{BY}{BY'}\cdot\frac{KY'}{KY}.$$

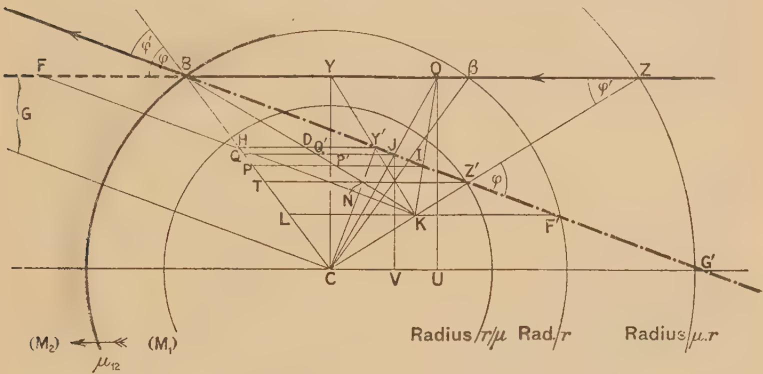


FIG. 7.—REFRACTION AT A SPHERICAL SURFACE.

Since the triangles KYC, Y′BC are similar, KY/CY=BY′/BC. Also the triangles KY′C, CYB are similar. ∴ KY′/CY′ =BY/BC.

$$\therefore \frac{KY'}{KY}\cdot\frac{CY}{CY'}=\frac{BY}{BY'}. \quad \therefore \frac{KY'}{KY}=\frac{1}{\mu}\cdot\frac{BY}{BY'}.$$

Also $$\frac{FB}{BF'}=\frac{KF'}{FK}=\frac{BY}{BY'}\cdot\frac{KY'}{KY}. \quad \therefore \frac{BF}{BF'}=-\frac{1}{\mu}\cdot\frac{BY^2}{BY'^2}.$$

(x.) $$\frac{KF'}{BF'}=\frac{NZ'}{BZ'}=\frac{NZ'}{\mu\,.\,TZ'}. \quad \therefore \frac{NZ'}{TZ'}=\mu\,.\,\frac{FB}{BF'}=\frac{BY^2}{BY'^2}.$$

(xi.) For BF, BF′, BO, BI, write $f$, $f'$, $u$, $v_1$ respectively. And for BY put $y$ or $r\,.\cos\varphi$, and for BY′ substitute $y'$ or $r\,.\cos\varphi'$.

$$\frac{1}{KF'}+\frac{1}{y}=\frac{1}{DY'}=\frac{1}{HY'}\cdot\frac{y'^2}{y^2}=\frac{\mu y'}{y^2}.$$

$$\therefore \frac{1}{BF}=\frac{y-\mu\,.\,y'}{y^2}. \quad \therefore \frac{1}{f}=\frac{\cos\varphi-\mu\,.\cos\varphi'}{r\,.\cos^2\varphi}.$$

$$\frac{1}{BF'}=-\frac{1}{BF}\cdot\frac{1}{\mu}\cdot\frac{y^2}{y'^2}=\frac{\mu\,.\,y'-y}{\mu\,.\,y'^2}. \quad \therefore \frac{1}{f'}=\frac{\mu\,.\cos\varphi'-\cos\varphi}{\mu\,.\,r\,.\cos^2\varphi'}.$$

(xii.) From Fig. 7 (writing $p$, $p'$ for IP, IP′ respectively), $1/p'=1/u+1/\mathrm{KF}'$.

$$\therefore \frac{y'^2}{y^2}\cdot\frac{1}{p}=\frac{1}{u}-\frac{1}{f}. \qquad \therefore \frac{y'^2}{y^2}\cdot\frac{\mu}{v_1}=\frac{1}{u}+\frac{\mu\,.\,y'-y}{y^2}.$$

$$\therefore \frac{\mu\,.\cos^2\varphi'}{v_1}-\frac{\cos^2\varphi}{u}=\frac{\mu\,.\cos\varphi'-\cos\varphi}{r}.$$

19. *Spherical Surface. Sagittal Rays.*

(xiii.) Draw (Fig. 8) CG′ parallel to BZ, cutting BZ′ in G′. Take a point $B_6$ on the spherical refracting surface so that a line through it drawn parallel to BZ makes an angle $\varphi$ with the normal at $B_6$. Join $B_6C$, $B_6G'$. Then the angle $CB_6G'$ is

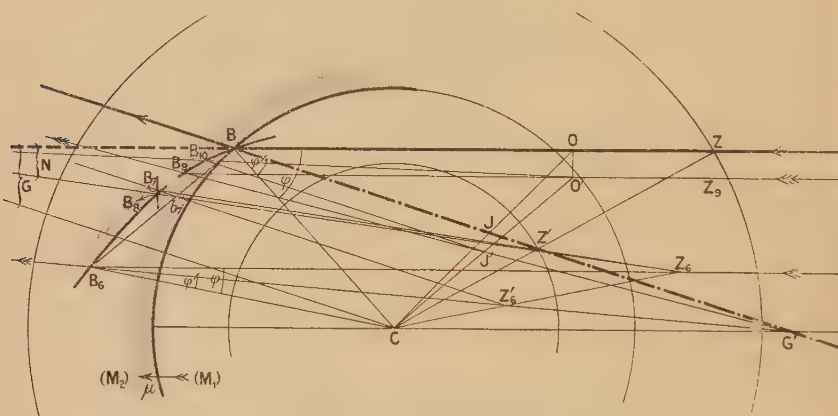


FIG. 8.—REFRACTION AT A SPHERICAL SURFACE. SAGITTAL RAYS.

equal to $\varphi'$, and hence $B_6G'$ is the refracted direction of the sagittal incident ray parallel to BZ. Similarly, the refracted direction of any ray of the sagittal pencil that is incident parallel to BZ, passes through G′.

For the ray $B_6Z_6$ let the aplanatic pair of points be $Z_6$, $Z_6'$. Through $Z_6'$ draw $B_7b_7Z_6'$, parallel to BG′, cutting the spherical surface at $B_7$ and the chord $B_6B$ at $b_7$. Draw $b_7Z_6$, cutting BZ at N and the surface at $B_8$. Draw GC parallel to BG′, cutting BZ at G. Then

$$NB/B_6Z_6=b_7B/B_6b_7=Z_6'G'/B_6Z_6'=CG'/B_6Z_6.$$

$$\therefore \ NB=CG'=GB, \text{ or N coincides with G.}$$

(xiv.) When $B_6$ is very close to B, the points $B_8$, $B_7$, $b_7$ sensibly coincide. Also the chord $B_6B$ very nearly coincides with the arc $B_6B$, and the geometrical relationships deduced from the finite chord become optical relationships for rays of light refracted at the infinitesimal arc. The above relations are therefore approximately true for a small arc.

(xv.) Suppose OO′ is a position of a very small object moving between BZ and $B_9Z_9$. Suppose these lines to be two incident sagittal rays and BG′, $B_9G'$ the respective refracted directions. Join GO′, cutting the chord (and arc) $B_9B$ at $B_{10}$. Draw $B_{10}J'$ parallel to BG′, cutting $B_9G'$ at J′. Then J′ is the image of O′.

Join CJ′, also J′O′. Then

$$B_9O'/GB = B_9B_{10}/B_{10}B = B_9J'/J'G'.$$

Also CG′ is parallel to $B_9Z_9$ and equal to GB. Therefore, CJ′O′ are collinear.

Then for any position of the object along BZ the image is at the point J, where OC crosses the direction of the refracted ray BG′.

Consider the object to move from the extreme right to the extreme left. Then G, G′ are identified as the first and second principal foci for narrow sagittal pencils having BZ for their chief ray.

(xvi.) In Fig. 7, $\frac{GB}{BO}=\frac{CG'}{BO}=\frac{JG'}{BJ}=\frac{BG'-BJ}{BJ}$. Write $v_2$ for BJ and $g$, $g'$ for BG, BG′ respectively. Therefore, $g/u+g'/v_2=1$.

(xvii.) Let BZ cut the circle of radius $r$ at B and $\beta$.

Then $BZ=r\,(\mu\,.\cos\varphi'+\cos\varphi)$ and $\beta Z=r\,(\mu\,.\cos\varphi'-\cos\varphi)$.

Also $-g=GB=CG'=\mu\,.\,Z'G'=\mu\,.\,g'-\mu\,.\,BZ'=\mu\,.\,g'-BZ$.

$\therefore\ g+\mu\,.\,g'=BZ=r\,(\cos\varphi+\mu\,.\cos\varphi')$.

The triangles BCG′, $Z\beta C$ are similar. Therefore,

$CG'/BC=C\beta/\beta Z$. $\therefore\ -g=r^2/\beta Z=r/(\mu\,.\cos\varphi'-\cos\varphi)$.

And $BG'/BC=CZ/\beta Z$. $\therefore\ g'=\mu\,.\,r/(\mu\,.\cos\varphi'-\cos\varphi)$.

Write $q$ for QJ. Then $1/q=-1/g+1/u$.

$\therefore\ \mu/v_2-1/u=-1/g=(\mu\,.\cos\varphi'-\cos\varphi)/r$.

20. *To find points on the Caustic formed by Refraction at a Spherical Surface.* (No figure.)—From C draw concentric circles of radii $r$ (representing the surface), $\mu r$, $\mu r/2$, $r/\mu$,

$r/2\mu$. (For convenience, keep one pair of compasses adjusted to radius $\mu r/2$, another pair to $r/2\mu$.)

From the radiant O draw any incident ray giving B, Z; draw CZ, giving Z′, also W, W′ where CZ cuts the circles of radii $\mu r/2$ and $r/2 . \mu$ respectively. With W as centre, radius $\mu r/2$, draw a circle cutting the incident ray BZ in Y; similarly, with W′ as centre, radius $r/2\mu$, find Y′ on the refracted ray BZ′. (Since WY=CW=WZ. ∴ CYZ is the angle in a semicircle. Similarly, CY′ is perpendicular to BZ′.) Draw YY′, obtain K. Join KO, obtain I. Join CO, obtain J.

Draw other incident rays and repeat the construction.

## ABSTRACT.

The Paper contains a development of optical imagery based on elementary geometry, including limiting positions, but excluding cross-ratios, centres of perspective, &c. The method adopted is useful for teaching the properties of optical systems to those who are not essentially students of pure mathematics, and can be very satisfactorily used by those capable of draughtsmanship with mathematical instruments.

The principle of the method is as follows: The vertex (object) of a small incident pencil of rays is considered to move along a line parallel to the principal axis of the optical system. This line is also regarded as the incident portion of a fixed ray. Then the directions of the ray after reflection or refraction at the several surfaces do not alter as the position of the object changes. They are therefore the fixed loci of the images or vertices of the reflected and refracted pencils. Then the successive positions of the image resulting from a progressive movement of the object are obtained. Also the constructions are such that in the case of a thick lens the importance of the cardinal points and planes is demonstrated.

First, considering refraction at a spherical surface and assuming that $\mu$ is the relative velocity of light in the two media, a construction for the refracted portion of any incident ray is obtained from the wave principle; then the sine law of refraction and the position of the aplanatic pair of points are deduced.

When the incidence is normal it is convenient in drawing to increase largely the scale of length perpendicular to the principal axis in comparison with that along the principal axis. A distorted figure is obtained, but the relative lengths of lines perpendicular to the axis or of lines parallel to the axis will be maintained.

The principal foci for normal incidence on a single spherical refracting surface are obtained, and then the usual algebraical expressions are deduced from the figure. A convenient construction for finding the principal foci and aplanatic pair of points when $\mu$ and $r$ are given is then shown.

For a thick lens it is most convenient to assume that the two surfaces separate three different media. After obtaining the refracted portions of a ray—called the second fixed ray—incident

parallel to the principal axis of the system, the positions of the cardinal and nodal points and planes are indicated and their more important properties deduced. A convenient construction is shown for finding the cardinal and nodal points, when $r_1$, $r_2$, $\mu$, $\mu'$ are given. At the same time the course of the first fixed ray is determined. The first fixed ray is defined as the ray which emerges from the system in the same line as the incident portion of the second fixed ray obtained above. There is also a visualisation of the Gauss and Abbe definitions of focal length. The usual algebraic relations are deduced. After a discussion of magnification—lateral, axial and angular—it is shown that when the six cardinal points of one optical system are combined with those of another a resulting set of six cardinal points is obtained, and thus a composition of the cardinal points of various optical systems may be effected.

Returning to refraction at a single spherical surface the cases of meridian and sagittal rays are separately considered. For the former the position of Cornu's junction point is obtained in a simple manner. Finally, the usual algebraic relationships are deduced and a graphic construction for points on a caustic is given.

## XVI. *Alternating-Current Magnets.* *By Prof.* E. Wilson.

Read February 28, 1913.

(Abstract.)

It follows from the well-known law of pull of an electromagnet that if the magnetic field alternates between positive and negative values the pull is unidirectional and intermittent. Unless means are provided to reduce the consequent chattering and vibration, the magnet is rendered useless. In the present experiments a phase-splitting device has been adopted, and consists in surrounding a portion of the pole-piece of the magnet with a short-circuited coil. The portion of the pole-piece so surrounded is sometimes said to be "shaded," and the coil referred to as a "shading" coil. The effect of this coil is to alter, not only the relative amplitudes, but the phase of the magnetic fields passing through the shaded and unshaded portions of the pole-face. The magnet used in the experiments varies the length of its gap when in action, and the influence of the gap length upon this phase displacement has been studied. When the resistance of the shading coil is such that the magnetic induction **B** over the whole face is substantially uniform and the gap closed, the phase displacement was 72 electrical degrees (360 deg. = 1 period). A gap length of 0·15 cm. reduces the phase-displacement to 18 deg., and consequently the minimum or "hold on" pull drops. This minimum or "hold on" pull is, of course, smaller than the average, and has to be taken into consideration in the design of the magnet. The arrangement of the shading coil above described is very effective in preventing vibration and chattering when the magnet is closed, and renders the alternating-current magnet a practical success.

With constant alternating voltage impressed upon the magnetising coils of the magnet the net pull exerted diminishes rapidly at first as the gap length increases, and tends to become more nearly constant. The R.M.S. amperes, on the other hand, steadily increase as the pull diminishes, owing to the increase in the gap length.

The observed net pull in the case of the magnet experimented upon is less than the calculated average pull, varying from 83 to 59 per cent. as the gap length varies from 0 to 1 cm.

## DISCUSSION.

Mr. T. Harding Churton asked at what frequencies the result had been obtained, as the chattering would, of course, be greatest at low frequencies. He was surprised to see that such a large displacement as 72 deg. could be obtained due to shading.

Prof. E. Wilson, in reply, stated that the frequency used was 50. The large displacement of phase only occurred when the air-gap was small, but it was only when the armature was actually in contact with the pole that it was required to abolish the chattering.

# XVII. *The Latent Heat of Evaporation of Aqueous Salt Solutions.*

*By* ROBERT G. LUNNON, *B.Sc.*, *University College, London.*

RECEIVED MARCH 5, 1913. READ MARCH 14, 1913.

THE question of the amount of heat required to evaporate 1 gramme of steam from an aqueous salt solution appears to have been dealt with experimentally by only one previous worker.* The results then obtained by Prof. Trouton were not entirely satisfactory, and at his suggestion the subject has been further investigated; the results, together with certain theoretical relations, are given in this Paper.

The cases in which the solution is a saturated one, and when it is unsaturated, require separate treatment. We shall denote by L the amount of heat, measured in calories, which is required to evaporate 1 gramme of steam from a salt solution of maximum concentration boiling under atmospheric pressure. The salt set free by the evaporation of its solvent remains undissolved at the same temperature. We shall use Q to denote the amount of heat required to dissolve a sufficient quantity of salt in 1 gramme of water, at the temperature of the boiling saturated solution, so that the resulting solution will be a saturated one.

With unsaturated solutions, evaporation by boiling is inevitably accompanied by an increase in the concentration of the solution, and if the boiling continues by a rise in its temperature. We shall therefore write $L_x dm$ for the total amount of heat required to evaporate $dm$ grammes of steam, minus the heat accounted for by the rise in temperature of the solution; the latter is supposed to contain $x$ grammes of salt to 1 gramme of water, and to be boiling under atmospheric pressure at $t$°C. This definition assumes that the heat required is proportional to the amount of steam evaporated, as long as that amount is small.

## *Experimental.*

The principle adopted for the measurement of L involved the supply of heat electrically, at a known rate, to a boiling solution, and the measurement of the rate of formation of steam by condensing and weighing at definite intervals. Heat

* F. T. Trouton, "Trans." Royal Irish Academy, Vol. XXXI., p. 345.

loss by radiation was checked by surrounding the whole calorimeter with a solution boiling at the same temperature.

Fig. 1 is a diagram of the apparatus used. The calorimeter A was supported on glass legs inside a double-walled vessel* B, closed by a large rubber cork ; and B was totally immersed in a solution contained in the large vessel C. The electric current was supplied by the leads W to an ordinary carbon filament lamp, L, enclosed in a copper jacket. This proved to be an efficient heating agent, about 400 calories per minute being developed from an 80-volt circuit. When the nitrates of

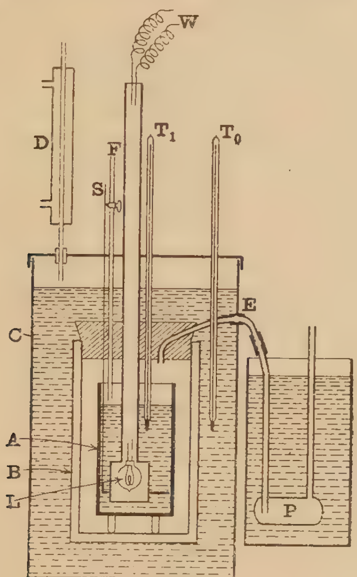


FIG. 1.

sodium and potassium, which are very soluble at high temperatures, were being used, a ring stirrer, S, was added to prevent the salt caking round the lamp, and a feeding tube, F, was also fitted so that when the solution became thick more readings might be obtained, without dismantling the apparatus, by adding water and so re-dissolving the free salt. The steam was led out by the exit tube E, remaining immersed in the bath until condensed in the detachable vessel P, which was im-

* A vacuum-walled flask would have been ideal for the purpose of the vessel B, but repeated trials showed that such a vessel was not sufficiently strong to bear the continued heating and cooling for long.

mersed in cold water. Thermometers $T_0$ and $T_1$ indicated the temperatures of the bath and calorimeter respectively, and a condenser, D, attached to the cover of the bath, enabled the temperature of the contents of the latter to be kept constant. The electric current was measured by an ammeter, and a voltmeter was placed across the leads of the lamp. This arrangement made it necessary to multiply the current, as recorded by the ammeter, by the fraction $\frac{6058}{6058+231}$ to obtain the current passing through the lamp, since the resistances of voltmeter and lamp were 6,058 ohms and 231 ohms respectively. Both instruments were carefully calibrated twice during the work and the efficiency of other parts of the apparatus was verified.

In making an experiment, about 200 cubic cms. of a hot concentrated solution of the pure salt were placed in the calorimeter, and the bath C was then usually filled with a solution of the same salt, boiling at the same temperature. In some cases, however, an unsaturated solution of some other kind was used, its strength being adjusted to give the required boiling point. Steam, of course, came off from the inner solution at this same temperature, and this fact ensured that no water was carried over with it into the condenser. The great difficulty of wet vapour, which, as A. W. Smith* has recently proved, has disturbed much previous work on latent heats, was thus automatically avoided.

The only serious uncertainty arose from the difficulty of keeping the temperatures of calorimeter and bath exactly the same. They sometimes differed by as much as 0·5°C., and the amount of heat passing through the walls of the calorimeter was then considerable. The difficulty was much more serious when unsaturated solutions were being used, for then the constant evaporation involved a continual rise in the temperature of the solution, often at the rate of 1 deg. in 20 minutes, and the temperature of the bath could not always be raised at the same rate. It was met by determining exactly, by two different methods, the heat lost or gained, when the two temperatures differed by observed amounts. In the one method, pure water was placed in the calorimeter, and a dilute solution outside, and the consequent slightly higher temperature of the bath caused to evaporate a certain amount of steam, which was measured in the usual way. In the second method, castor oil,

* A. W. Smith, Phys. Rev., 33 (1911), p. 173.

undergoing no appreciable change when heated for a short time, was placed inside, and was kept at a temperature slightly higher than that of the bath by a small supply of current. The heat leak was thus measured by the heat so generated.

Experiments at various times by these methods gave fairly consistent results, which showed that for differences of $\pm 2°C.$ the heat leak was proportional to the temperature difference, its magnitude being 9 to 10 calories per degree per minute. The irregularity due to this cause, if uncorrected, might amount to $\frac{1}{2}$ per cent., and when allowance according to scale had been made it was much less. Greater accuracy than this, however, cannot be claimed for the results as a whole. The boiling of a concentrated solution is not a simple phenomenon, especially when it contains three or four parts by weight of salt to one of water; and the variation of uncertain conditions made it no easy matter to obtain consistency in the results. Experiments with pure water were made with the apparatus, and the results were very good, giving values of L between 539 and 541.

A set of readings which were obtained with a saturated solution of KCl will illustrate the method. The first column gives the time, the last two the weights of the condenser and of the steam evolved per minute.

Jan. 5th, 1912. Current: 0·245 amperes. Voltage: 79·9 volts.

| $t$ | $T_1$ | $T_0$ | W | $w$ |
|---|---|---|---|---|
| h. m. s. | | | | |
| 12 19 19½ | 108·5°C. | 109·4°C. | 115·409 g. | ... |
| 0 26 19½ | 108·6 | 109·2 | 119·376 | 0·555 g. |
| 0 38 3½ | ,, | 109·1 | ... | ... |
| 0 46 3½ | ,, | ,, | 123·857 | 0·558 |
| 0 48 12 | ,, | ,, | ... | ... |
| 1 0 12 | 108·65 | ,, | 130·510 | 0·554 |
| 3 43 39½ | 108·6 | 108·8 | 108·763 | ... |
| 0 43 39½ | ,, | ,, | 114·357 | 0·559 |
| 0 45 35 | ,, | ,, | ... | ... |
| 0 55 35 | ,, | ,, | 119·935 | 0·558 |

$$\text{Heat supply}=\frac{\text{EC}}{\text{J}}=14{\cdot}31\times79{\cdot}9\times0{\cdot}245\times\frac{6058}{6289}=270 \text{ calories per minute.}$$

Heat leak from bath = 4 calories.

$$\therefore \text{ Mean L}=\frac{274}{0{\cdot}557}=492 \text{ calories per gramme.}$$

The final means of large numbers of experiments on solutions of six common salts are given in the following Table, together

with the boiling points (T) of the solutions, their strengths ($x$) in grammes per gramme of water, the latent heat of pure water at the temperature T ($L$ ), the heat of solution as given by the difference $L_T - L$, and the molecular weights (M) of the salts. The Table is arranged in descending order of maximum boiling points.

| Salt. | T | L | $L_T$ | $Q = L_T - L$ | $x$ | M |
|---|---|---|---|---|---|---|
| $NaNO_3$ | 121·0°C. | **459** | 525 | 66 | 2·18 | 85 |
| $KNO_3$ | 116·8 | **421** | 528 | 107 | 3·38 | 101 |
| NaCl | 110·0 | **508** | 533 | 25 | 0·40 | 58 |
| KCl | 109·0 | **493** | 533 | 40 | 0·59 | 74 |
| $K_2CrO_4$ | 106·8 | **505** | 535 | 30 | 0·82 | 194 |
| $K_2Cr_2O_8$ | 104·8 | **489** | 537 | 48 | 1·03 | 294 |

These results are illustrated in Fig. 2, in which the heat of

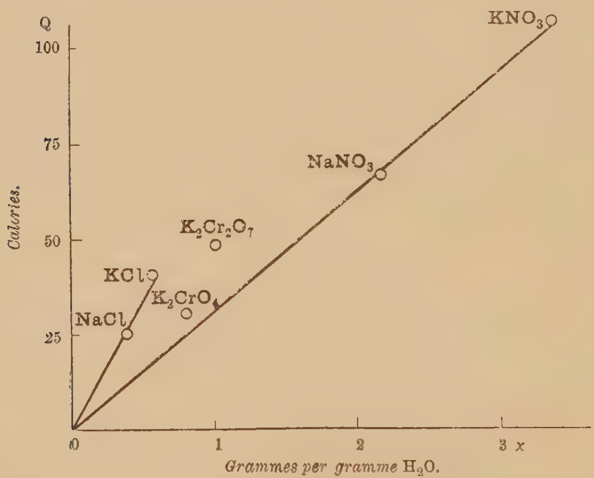


FIG. 2.

solution Q is plotted against the saturation concentration $x$. The most obvious connection is that between the points corresponding to salts of the same acid. The chlorides and the nitrates show definitely similar relationships, and it seems probable that for salts of the same acid Q is proportional to $x$, as is indicated by the two lines drawn on the diagram. The present results are not sufficient in number to test this suggestion, and, moreover, they can only be considered to be correct to $\pm 2$ calories.

Some relation might be expected to exist between the heat of solution and the strength of the solution in gramme-molecules. Fig. 3 gives the variation of Q with $x/M$, and it is less regular than the previous figure. This is not surprising, since the degree of dissociation of the salts would be as important as the number of molecules in solution, and this would be different for the different solutions. Unfortunately, no data exist for the dissociation coefficient, if there be such, of strong solutions.

Experiments with *unsaturated solutions* were made with two salts only, $KNO_3$ and $NaNO_3$. In deducing the value of $L_x$ from the experimental observations we use the relation

$$JCE+h=mL_x+WdT,$$

where $h$ is the heat obtained by leakage from the outside bath, $m$ is the weight of steam formed per minute, W the water

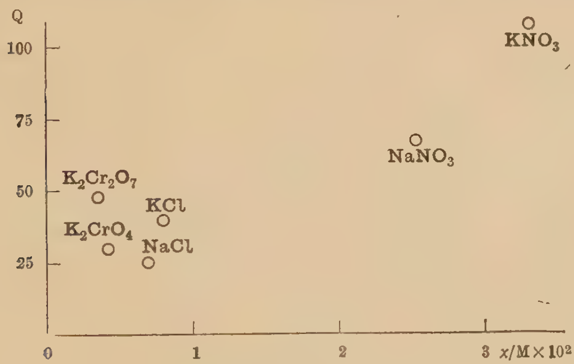


FIG. 3.

equivalent of the calorimeter and its contents, and $dT$ the rise in temperature consequent upon the increase in concentration. This is in accordance with the definition of $L_x$ which we have adopted, since $m$ is always very small compared with the total mass of solution present. W is a number of considerable uncertainty, as there are no data available for the specific heat of strong solutions at high temperatures; but its value in each case was deduced in an obvious manner from special observations of the rate of rise of temperature before boiling began. Moreover, it has the small quantity $dT$ as its factor in the equation, and an approximate value is, therefore, sufficient.

The measurements were made in the same way as for saturated solutions. The calculated values of $L_x$ are given in the

following Table for solutions of different strengths and boiling points. The numbers given for $x$ and T are of course the mean values of these quantities over the range covered by the boiling solution during the experiment, which, as regards temperature, was usually of the order of half a degree.

| $NaNO_3$ | | | $KNO_3$ | | |
|---|---|---|---|---|---|
| T | $x$ | $L_x$ | T | $x$ | $L_x$ |
| 100°C. | 0 | 540 | 100°C. | 0 | 540 |
| 106 | 0·58 | 553 | 103 | 0·47 | 541 |
| 108 | 0·78 | 550 | 106 | 1·01 | 541 |
| 110 | 0·99 | 554 | 110 | 1·88 | 543 |
| 112 | 1·21 | 547 | 113 | 2·74 | 540 |
| 115 | 1·56 | 545 | 116 | 3·20 | 543 |
| 121 | 2·18 | 459 | 116·8 | 3·38 | 421 |

The values of L for the saturated solution have been added in the last line. They emphasise the striking result that, as long as the solution remains unsaturated, the quantity of heat required to evaporate a gramme of steam from it is very approximately *constant*; but as soon as saturation is reached the amount required drops by about one-fifth of its former value. In the case of $KNO_3$ the constancy of $L_x$ is remarkable, while for $NaNO_3$ the variations are irregular and are within the limits of experimental accuracy. The explanation of the final drop in value is to be found in the fact that in the last stage salt separates out from the solution as evaporation proceeds, while it does not do so before that stage is reached; but it is difficult to give a reason for the constancy of $L_x$ while $x$ increases, in the case of $KNO_3$, from 0 to 3·2.

*Theoretical.*

The quantity L which we have measured can be connected with other thermal constants by considering the two following thermodynamic cycles.* In each we commence with a quantity of saturated solution containing 1 gramme of water at its boiling point $t$°C. under atmospheric pressure $p$; and in the first cycle the processes are isothermal throughout.

* The diagrams accompanying the cycles do not completely represent them, since the changes are such as to involve different parts of the working substance, being at different temperatures from one another during some of the steps; and when this occurs one point cannot completely represent the system.

*First Cycle.*

1. AB. Evaporate 1 gramme of steam from the solution. The heat required is L; and if $w$ be the decrease in volume of the solution and solid salt and $v$ the consequent volume of steam, the work done by the substance is $p(v-w)$.

2. BC. Separate the steam and compress it isothermally until the saturation pressure $p_1$ is reached. The work done is $\int_p^{p_1} pdv$, and the heat required is approximately $J\int_p^{p_1} pdv$. This, of course, is assuming that steam behaves as a perfect gas, but the error is actually less than $\frac{1}{2}$ per cent.

3. CD. Condense the steam. The heat required is $-L_t$ (the latent heat of evaporation of water at $t$°C.), and the work done is $p_1(u_1-v_1)$, where $u_1$ is the volume of 1 gramme of steam at pressure $p_1$ and temperature $t$.

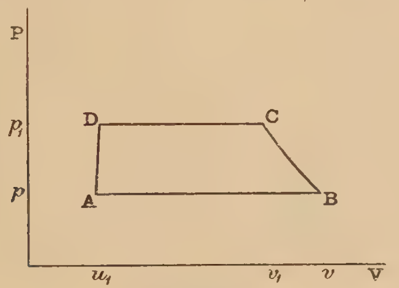


DIAGRAM 1.

4. DA. Dissolve the salt set free in the first stage, in the water obtained in the third stage. The heat required is Q, while the work done depends upon the change of volume on solution, and we shall obtain an approximate result by neglecting it.

Equating now the heat required and the work done, we have

$$L+Q-L_t=\frac{1}{J}[(pv-p_1v_1)+p_1u_1-pw].$$

We have already assumed that $pv=p_1v_1$, and the quantity $p_1u_1-pw$ is equivalent to about 0·02 calorie, $p_1$ being approximately two atmospheres. We may, therefore, neglect the right-hand side of the equation, and obtain the approximate result

$$L=L_t-Q. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

*Second Cycle.*

1. AB. Evaporate 1 gramme of steam from the solution. This requires a quantity of heat L, and the work done is $p(v-w)$ as before.

2. BC. Cool the steam at constant pressure to 100°C., *i.e.*, until it is saturated, and let its volume then be $v_0$. The heat required is $-\int_{100}^{t}\sigma dt$, where $\sigma$ is the specific heat of steam at constant pressure, and the work done is $p(v_0-v)$.

3. CD. Condense the steam at 100°C. If $u_0$ denote the volume of 1 gramme of water at 100°C. the work done is $p(u_0-v_0)$ and the heat required is $-L_{100}$, the latent heat of water at 100°C.

4. DE. Heat the water to the original temperature $t$, increasing the pressure so that equilibrium always exists. Let

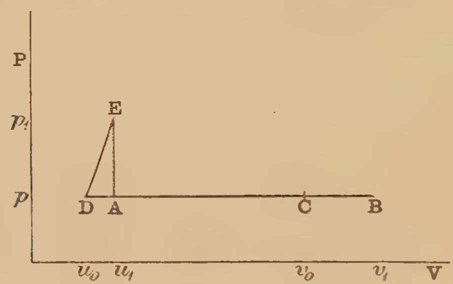


DIAGRAM 2.

the final pressure be $p_1$ and the volume $u_1$, and write $s$ for the specific heat of water. Then the work done is $\int_{u_0}^{u_1} pdu$, and the heat required is $\int_{100}^{t} s\,dt$.

5. EA. Finally, re-dissolve the salt in the water, so that the whole is in the same condition as at first. The heat required is Q, and, as in the first case, we shall obtain an approximate result by neglecting the work done.

Adding the various items and simplifying we obtain

$$L+Q-L_{100}+\int_{100}^{t}(s-\sigma)dt=\frac{1}{J}\left[p(u_0-w)+\int_{u_0}^{u_1}pdu\right].$$

The value of the terms on the right-hand side is a small fraction of a calorie in all practical cases, and for our present purpose

may be omitted. If, further, we take a mean specific heat over the range from 100°C. to $t$°C., defined by $s_m(t-100)=\int_{100}^{t} s\,dt$, and similarly for $\sigma$, we obtain the result

$$L=L_{100}-Q+(\sigma_m-s_m)(t-100). \quad . \quad . \quad . \quad (2)$$

Another relation is obtained if we make the second cycle a reversible one, so that the Second Law can be applied. Reversibility will be ensured if the solution, in the fifth stage, be made to take place through a semi-permeable membrane; and we shall need to define a new heat of solution, Q′, which is required under these conditions. The expression of the fact that the total change in entropy is zero is now easily found to be

$$L=\frac{T}{T_0}L_{100}-Q'+(\sigma'_m-s'_m)T\log\frac{T}{T_0}, \quad . \quad . \quad . \quad (3)$$

where T, $T_0$ are the temperatures $t$°C., 100°C. on the absolute scale, and $\sigma'_m$, like $s'_m$, is a new mean specific heat, defined by $\sigma'_m\log\frac{T}{T_0}=\int_{T_0}^{T}\frac{\sigma}{T}dT$.

The connection between Q and Q′ is easily found in terms of the osmotic work. The difference, Q′−Q, is equivalent to the work done by the substance, first, during the compression of the salt to the pressure P, the osmotic pressure of the saturated solution, and, second, during the movement of the osmotic membrane. If the decrease in the volume of the salt, when the pressure P is applied, be denoted by $d$, the first part of the work is $-Pd$. If $w$ is the increase in the combined volume of salt and solution when 1 gramme of water is added, the osmotic work done is practically $+Pw$, for the liquid *behind* the membrane is always saturated, and therefore exerts the pressure P throughout the process of solution. We have then the approximate relation

$$Q'-Q=\frac{1}{J}P\,.\,(w-d). \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

A result of some interest follows at once from equations (1) to (4), viz.:—

$$P(w-d)=J\left\{L_{100}\left(\frac{T}{T_0}-1\right)+(\sigma'_m-s'_m)T\log\frac{T}{T_0}\right.$$
$$\left.-(\sigma_m-s_m)(T-T_0)\right\}. \quad . \quad (5)$$

In the case of an incompressible salt, the left-hand side of (5) represents the osmotic work for the solution. On the right-hand side, the only term which relates to the solution is T, the maximum boiling point; the specific heats depend only upon T also. If, therefore, the solutions of two salts have the same maximum boiling point at the same pressure, the osmotic works for the two solutions are approximately equal, and the osmotic pressures are of the same order, independently of the nature of the salts and of the concentration of their saturated solutions. Pairs of salts for which this is true are by no means uncommon, examples being the nitrate and tartrate of potassium, both boiling at 105·0°C., and potassium carbonate and zinc sulphate, both boiling at 108·0°C. We can, moveover, find the value of the osmotic pressure for a solution whose maximum boiling point T is known by substituting their appropriate values for the terms in (5). Thus, for the case of sodium nitrate, boiling at 121°C., the values of $s$ and $\sigma$ can be calculated from Tables, and the result obtained for the osmotic work is $1{\cdot}68\times10^9$. The value of $w$ is not much different from unity, and the osmotic pressure is, therefore, of the order of $1{\cdot}68\times10^9$ ergs, *i.e.*, about 1,680 atmospheres.

It is not at present possible to test our experimental results with the theoretical relations (1) to (5) which we have developed. Equations (1) and (2) involve the quantity L+Q, and its elimination leads to the approximate result for pure water

$$L_t=L_{100}+(\sigma_m-s_m)(t-100).$$

This is known to be true for moderate values of $t$, such as we have assumed in making approximations. Absence of other thermal data hinders the application of these equations to the experimental results. One measurement of Q is, however, available for comparison, this having been obtained by Mr. A. W. Anscombe, B.Sc., who has commenced an investigation of the subject at this college. In his experiments, water at a high temperature and pressure is allowed to pass into a vessel containing salt at the normal boiling point ($t$°C.) of the saturated solution, and the initial temperature of the water is adjusted so that the final temperature of the solution formed is exactly $t$°C. The value of Q so obtained for $NaNO_3$ is 64·5 calories per gramme of water; and this is in good agreement with the value 66 obtained from the measurement of L with the result of equation (1).

For unsaturated solutions, theoretical results can be ob-

tained connecting the quantity $L_x$ with the partial heat of solution and with the heat of dilution ; but for these quantities no experimental data are as yet available and theoretical results, such as those of Kirchhoff and Clapeyron, cannot be appealed to for solutions of such large concentrations as are here mainly dealt with.

Finally, I would here record my thanks to Prof. Trouton for suggesting this subject to me, and for encouragement during the work, and also to Mr. A. W. Porter, and to Mr. Anscombe for much helpful criticism.

## ABSTRACT.

The Paper records the results of a research into the latent heat of evaporation of steam from salt solutions.

The experimental method was to supply a measurable quantity of heat electrically, through a small lamp to the solution boiling inside a calorimeter. The latter was placed within a double-walled vessel surrounded by a solution boiling at the same temperature ; and the steam from the inner vessel passed out through a tube into a detachable condenser, which was weighed at intervals.

Measurements were made with the solutions of six different salts. Theoretical considerations show that the difference between the measured heat L, and $l_T$ the known heat of evaporation of water at the same temperature, is the heat of solution Q ; and the present results indicate that for salts of the same acid Q is proportional to the concentration.

For unsaturated solutions of KCl and NaCl the interesting result is found that the heat of evaporation is approximately constant for all concentrations until saturation is reached.

## DISCUSSION.

Mr. F. E. Smith remarked that in a similar apparatus he had used for the distillation of water he found that priming did take place. It would have been more satisfactory if the author had used a solution of NaCl and tested the distillate for its presence.

The Author stated that he had tested the distillate in one case, but not with NaCl.

## XVIII. *On Errors in Magnetic Testing due to Elastic Strain.* By ALBERT CAMPBELL, *B.A.*, *and* H. C. BOOTH, *A.R.C.Sc.* (*From the National Physical Laboratory.*)

RECEIVED FEBRUARY 14, 1913. READ APRIL 11, 1913.

SOME years ago, in the course of magnetic tests on sheet material, Mr. T. L. Eckersley and one of the authors noticed that considerable errors were sometimes introduced by even slight bending of the sheets or strips. Mr. Eckersley worked out the stresses, and these, in combination with the known effects of compression and tension,* were found to be sufficient to account in a general way for the observed results. Only a few systematic experiments were made at the time, but recently a more comprehensive series have been carried out. The following description of some of these will serve the object of the present Paper, which does not aim at absolute results, but rather indicates the order of the errors that may occur in various cases. Hence the reader must not be too critical of the methods employed, as their accuracy was sufficient for the purpose in view. Two methods, A and B, were used.

In method A a single length of strip, which was flat in the unstrained condition, was bent into ring form, care being taken to avoid permanent set as much as possible. The slightly overlapping ends were securely clamped, and the ring was evenly wound with secondary and primary coils. These windings were of thin wire arranged close to the strip so as to keep the ring as flexible as possible. While in this condition of temporary strain, the ring was tested for permeability and hysteresis by the ballistic method. The greater part of the temporary strain was then annulled by changing the circular form into a square by sharp bends at four places. The windings were not removed for this alteration. The ballistic tests were then repeated. As will be seen by the results given below, the changes observed were considerable. The effect of the bends at the corners (which would worsen the magnetic qualities) has been ignored, and the square form has been considered as giving the condition of " no strain."

In method B two strips of the material, with suitable uniform windings, were clamped flatwise at the ends by two solid yokes,

* *See* Ewing " Magnetic Induction in Iron and Other Metals," Chapter IX. (Third Edition, 1900).

forming with them a long rectangular magnetic circuit. This circuit could be tested with the strips flat or bent to any desired amount. Permanent set at various curvatures could be applied, and the strips could be restored to their original flat form after any of the bending processes.

Tests were made both on ordinary (*unalloyed*) soft transformer sheet and on silicon iron (containing about 3 per cent. of silicon). The latter is much the harder material, and showed correspondingly greater alterations due to temporary strain.

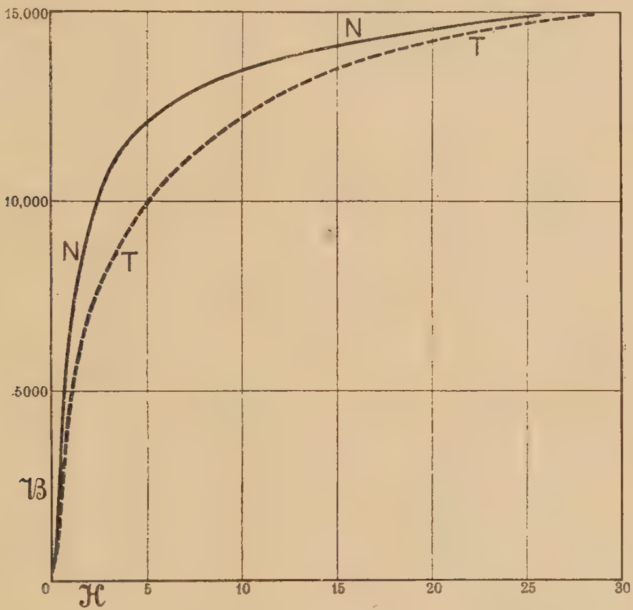


Fig. 1.—Permeability of Silicon Iron. Ring Method.
N=No strain. T=Temporary.

The accompanying curves and tables show some of the results obtained. The curves are marked in each case as follows :—

N, No strain.
T, Temporary strain.
P, Equal permanent strain.

Tables 1, 1A, 2 and 2A (with Figs. 1 and 2) refer to the ring method, and the others to the yoke method. In the latter method, after the effects of temporary strain had been tested, the strips were allowed to unbend back to the straight condition. When re-tested they gave practically the same results,

both for permeability and hysteresis, as in the original unstrained condition. This interesting fact is not indicated in in Fig. 3. It will be seen from that figure that the temporary

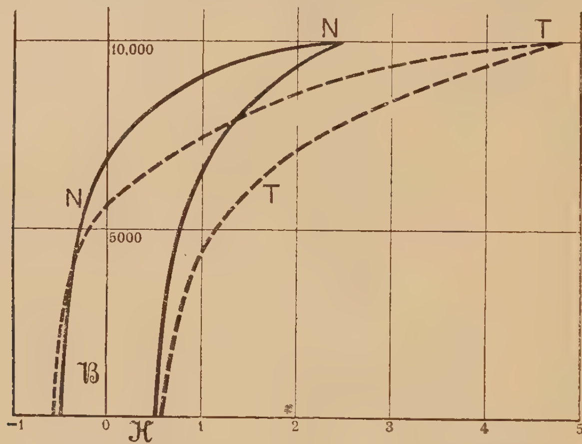


FIG. 2.—HYSTERESIS IN SILICON IRON: RING METHOD.
N=No strain. T=Temporary strain.

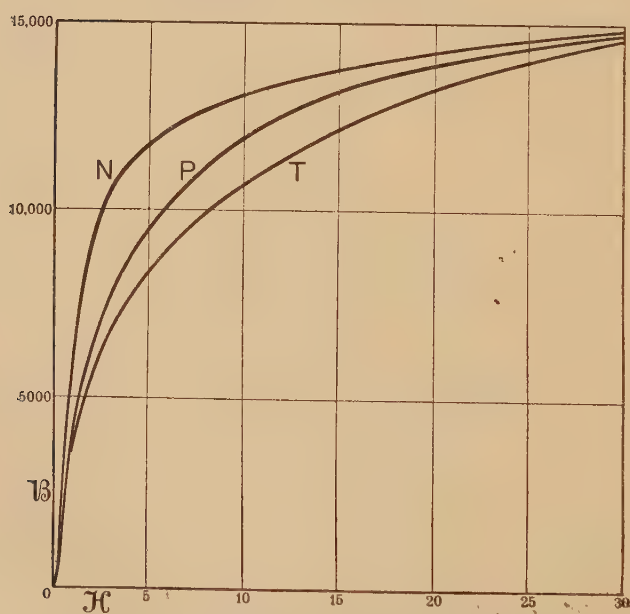


FIG. 3.—PERMEABILITY OF SILICON IRON. YOKE METHOD.
N=No strain. T=Temporary strain. P=Equal permanent strain.

strain has considerably greater effect on the permeability and the hysteresis than the equal permanent strain has.

TABLE 1.

Ordinary transformer iron (0·31 mm. thick). T, ring 23 cm. diameter; N, bent into square.

| $\mathcal{H}$ | N, no strain. | | T, temporary strain. | | $\mu_0/\mu_1$ |
|---|---|---|---|---|---|
| | $\mathfrak{B}$ | $\mu_0$ | $\mathfrak{B}$ | $\mu_1$ | |
| 1 | 1,800 | 1,800 | 1,390 | 1,390 | $1{\cdot}29_5$ |
| 2 | 5,780 | 2,890 | 4,600 | 2,300 | 1·26 |
| 5 | 10,080 | 2,016 | 8,980 | 1,796 | 1·12 |
| 10 | 12,340 | 1,234 | 11,780 | 1,178 | 1·05 |
| 20 | 13,880 | 694 | 13,780 | 689 | 1·01 |
| 50 | 15,530 | 311 | 154,80 | 310 | 1·00 |

TABLE 1A.

Same ring as in Table 1.

| $\mathfrak{B}_{max.}$ | Hysteresis loss. Ergs per c.c. per cycle. | | Increase per cent. |
|---|---|---|---|
| | N, no strain. | T, temporary strain. | |
| 5,000 | 1,117 | 1,214 | 8·7 |
| 10,000 | 3,628 | 3,968 | 9·4 |

TABLE 2.

Silicon-iron (0·31 mm. thick). T, ring 50 cm. diameter; N, bent into square.

| $\mathcal{H}$ | N, no strain. | | T, temporary strain. | | $\mu_0/\mu$ |
|---|---|---|---|---|---|
| | $\mathfrak{B}$ | $\mu_0$ | $\mathfrak{B}$ | $\mu$ | |
| 1 | 6,510 | 6,510 | 4,640 | 4,640 | 1·40 |
| 2 | 9,330 | 4,665 | 6,980 | 3,490 | 1·34 |
| 5 | 12,090 | 2,418 | 9,980 | 1,996 | 1·21 |
| 10 | 13,440 | 1,344 | 12,230 | 1,223 | 1·09 |
| 15 | 14,110 | 941 | 13,510 | 900 | $1{\cdot}14_5$ |
| 25 | 14,870 | 595 | 14,700 | 588 | 1·01 |

TABLE 2A.

Same ring as in Table 2.

| $\mathfrak{B}_{max.}$ | Hysteresis loss. Ergs per c.c. per cycle. | | Increase per cent. |
|---|---|---|---|
| | N, no strain. | T, temporary strain. | |
| 5,000 | 468 | 558 | 19·2 |
| 10,000 | 1,552 | 1,850 | 19·2 |

TABLE 3.

Silicon-iron (0·42 mm. thick) by yoke method.
N, strips straight.
T, bent temporarily to arc of circle 18 cm. radius.
P, bent with permanent set to arc of same circle.

| $\mathfrak{H}$ | N, no strain. | | T, temporary strain. | | P, permanent set. | |
|---|---|---|---|---|---|---|
| | $\mathfrak{B}$ | $\mu$ | $\mathfrak{B}$ | $\mu$ | $\mathfrak{B}$ | $\mu$ |
| 1 | 6,580 | 6,580 | 4,220 | 4,220 | 4,700 | 4,700 |
| 2 | 9,210 | 4,605 | 5,710 | 2,855 | 6,580 | 3,290 |
| 5 | 11,770 | 2,354 | 8,340 | 1,668 | 9,450 | 1,890 |
| 10 | 13,100 | 1,310 | 10,640 | 1,064 | 11,900 | 1,190 |
| 20 | 14,170 | 708 | 13,200 | 660 | 13,870 | 693 |
| 30 | 14,820 | 494 | 14,550 | 485 | 14,740 | 491 |

TABLE 3A.

Same sample as in Table 3.

| $\mathfrak{B}_{max}$ | Hysteresis loss. Ergs per c.c. per cycle. | | |
|---|---|---|---|
| | N, no strain. | T, temporary strain. | P, permanent set. |
| 10,000 | 1,458 | 1,834 | 1,754 |

It should be mentioned that the results here given are only examples of cases in which the effects were somewhat pronounced ; in some other samples which were tried the same amount of bending gave much less change. This is quite to be expected, for the total effect is a differential one, the permeability being sometimes increased by the tension and decreased by the compression resulting from the bending. It is, however, to the existence of the worse cases that attention has to be drawn.

In the Richter method of testing hysteresis loss the sheets are bent into the form of a ring of 50 cm. diameter. Although this gives a curvature which appears to be very moderate, the results given in Table 2A show that the slight bending may cause errors (in the hysteresis loss) of more than 10 per cent. From the above investigation the general conclusion follows that in all magnetic tests it is important that the greatest care should be taken to avoid not only permanent deformation but also temporary elastic strain.

## ABSTRACT.

In magnetic tests on sheet material considerable errors may occur if the sheets or strips are tested while in bent form. These errors, which are in general agreement with the known effects of compres-

sion and tension, were investigated experimentally with one or two forms of magnetic circuit similar to those sometimes occurring in practice. In one method a single length of the strip was bent into ring form with ends clamped together. This was wound with flexible primary and secondary coils, and tested for permeability and hysteresis, while in the condition of temporary strain. The temporary strain was then annulled by changing the circular form into a square by sharp bends at four places. The magnetic tests were repeated, and usually a considerable alteration was observed. For example, a silicon-iron ring 0·3 mm. thick and 50 cm. in diameter (the size used in Richter's method of testing hysteresis and eddy current losses) showed a decrease of 40 per cent. in the permeability for $\mathsf{H}=1$ due to the bending. The hysteresis loss was increased by 19 per cent. In another method the ends of two strips were clamped in yokes, and tests were made with different amounts of bending. It was found that temporary strain has considerably greater effect on the permeability than equal permanent strain has.

## DISCUSSION.

Prof. C. H. Lees pointed out that the authors had not clearly distinguished between the effects of stress and strain, and that the results would be more intelligible if this distinction were made. The condition called by them "temporary strain" was strain plus stress, while that called "equal permanent strain" was the strain without the stress. From the point of view of the molecular dynamical theory he thought the effects of stress ought to be separated from those of strain.

Dr. S. W. J. Smith expressed interest in the results and, referring to the difference between the effects produced by temporary stress and by permanent strain, thought there was no reason to expect any simple relation between them. Under different fields, in the same material, or under similar fields, in other materials, the effects might even be opposite to one another in sign.

Prof. T. Mather asked if the sheets were ever annealed after being bent, before testing them by Richter's method, as this would seem to remove the objections against the method.

Mr. A. Campbell, in reply, admitted the advisability of separating the effects of stress from those of strain. The advantage claimed for Richter's method was that it in no way mutilated the sheets. If they were annealed after they were bent into a cylinder this advantage would be destroyed.

# XIX. *Note on Cathodic Sputtering.* *By* G. W. C. Kaye, *B.A.*, *D.Sc.* *The National Physical Laboratory.*

Received February 13, 1913. Read April 11, 1913.

As is well known, if a current is sent through a discharge tube at low pressures, the glass adjacent to the cathode often becomes coated with a deposit of metal to an extent which depends on, among other things, the material of the cathode and the nature of the gas in the tube. The anode, on the contrary, shows little or no such effect.

This cathodic "sputtering" was noticed in the very early days of vacuum tubes: Plücker (1858) and Geissler remarked on it. Dr. Wright, of Yale, in 1877 used the method to platinise glass. The subject has been investigated by a number of workers, including Sir Wm. Crookes in 1891 ("Proc." Roy. Soc. A., 50, p. 88), Holborn and Austin, in 1903, at the Reichsanstalt ("Phil. Mag.," 8, p. 145), Houllevigue ("Ann." Chim. Phys., 1909-10). and Kohlschütter and his coadjutors, whose work is described in a recent series of Papers in the "Zeitschrift für Elektrochemie" (1906-1912).

The sputtered metal is shot off as small aggregates of molecules, apparently of the same order of magnitude as the particles in colloidal solutions. They appear to be projected approximately normally from the cathode, and to carry a negative charge, though, on account of their relatively large mass, the streams of metal are not deflected by a magnet to anything like the same extent as cathode rays. It does not appear that the disintegration of the cathode plays any appreciable part in the transmission of the current.

Cathodic sputtering has proved very useful as a means of preparing films and mirrors, especially in the case of metals intractable by the usual methods. It is found that the deposits only settle on surfaces which are positive with respect to the cathode. In practice this is secured by joining the surface to be plated to the anode.

The exact mechanism of the production of the sputtered particles is doubtful and has been the subject of a recent controversy. It is definitely established, however, that the amount of sputtering depends on:—

1. *The Nature of the Cathode.*

It is a rule, to which there is a number of exceptions, that

the amount of sputtering is roughly proportional to the chemical equivalent of the metal. To particularise, the effect in air with aluminium and iron is small. With palladium, platinum, gold, silver, copper, cadmium and tin sputtering is usually marked.

2. *The Temperature of the Cathode.*

If the temperature of the cathode be raised appreciably either by extraneous means or by the discharge itself, metals of high volatility, such as cadmium and zinc, have their sputtering properties greatly enhanced.

3. *The Nature of the Gas.*

Hydrogen, nitrogen and carbon dioxide are unfavourable to the cathodic disintegration of most metals. Oxygen and more especially the monatomic gases, helium, neon, argon, krypton, xenon and mercury vapour bring about marked sputtering of nearly all metals; argon is particularly efficacious in this respect. The nature of the gas furthermore controls the appearance and properties of the deposit.

4. *The Current Density in the Discharge Tube.*

The disintegration appears to be roughly proportional to the square of the current density, so that in sputtering experiments it is beneficial to use small (wire) electrodes and as large an induction coil as may be expedient.

5. *The Fall of Potential at the Cathode.*

The volatilisation of the cathode is augmented by increasing the potential on the tube and so increasing the potential drop at the cathode, and this is most readily controlled by regulating the pressure of the gas. Sputtering is much more pronounced at low pressures than at high, though it is possible to force the pressure too low.

An increase in the potential drop at the cathode also increases the range of projection of the particles.

It appears to be essential that the cathode-fall shall exceed a certain minimum voltage before the metal becomes ionised and disintegrated to any appreciable extent; and this fact is sufficient to reveal, under favourable conditions, a marked difference between the sputtering which occurs at the various parts of a cathode of irregular shape. The potential gradient

attains a maximum at points and edges; and accordingly it is at such places that the tendency for the metal to sputter predominates.

A discharge tube which the writer has used at the National Physical Laboratory illustrates this feature very strikingly. The electrodes were cylinders made by bending thin sheet aluminium so that two opposite edges came nearly together. These cylinders fitted very loosely within the tube, which was filled with helium—a gas somewhat favourable to the sputtering of aluminium. The pressure was not very low—about that which displays the helium spectrum to advantage—so that the tube ran easily, the dark space was small, and the potential applied was never great. As Fig. 1 indicates, there is no trace of sputtering at the anode, but an examination of the tube near the cathode shows that, while the glass facing the sides of the cylinder is untouched by the deposit, there is a brilliant black mirror on the glass immediately beyond each end of the cylinder.

Evidently the particles of metal were shot off exclusively from

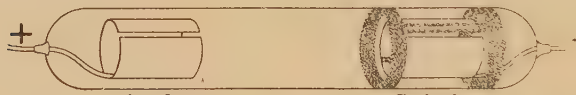


FIG. 1.

the region of the edges of the cylinder where the potential gradient reached a maximum. Elsewhere, the field attained was not strong enough to cause the emission of metal. This is borne out by the fact that there is a narrow band of deposit on the glass parallel to and directly facing the slit where the two edges of the bent plate come together. These two edges did not quite meet, and the intervening space is reflected in the band of the deposit which shows a streak of clear glass along the middle of its length. There is an extensive and ill-defined deposit on the inside of the aluminium tube opposite the slit. This is explained by the fact that the potential of the inside of the sheet is positive with respect to the edges.

Fig. 2 is a photograph of the sputtered deposit on the glass with the cathode removed. Fig. 3 shows the outer surface of the cylindrical cathode which has been flattened out for the purposes of the photograph; the extent of the stained active region round the edges is clearly discernible. Both Figs. 2 and 3 are full size.

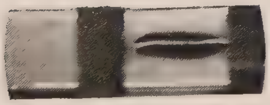

FIG. 2.

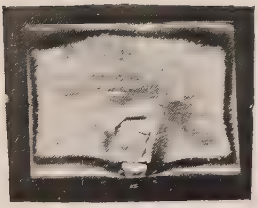

FIG. 3.

*To face page* 200.]

While the present case serves as an interesting illustration of the distribution of electric density on a cylindrical conductor, it may point a moral in X-ray tube construction. The ordinary potential-excess of the edges of a cathode of an X-ray bulb is accentuated by the disposition of the cathode which, for greater steadiness and hardness of the discharge, is always set back a little into a side tube. The glass walls of this tube become negatively charged by leakage from the cathode and accordingly the cathode rays are repelled to the centre of the cathode where they leave as a sharply defined pencil along the axis. This is the case whether the cathode is concave or plane. Now, since the centre of the cathode is the main source of the cathode rays which carry the current through the tube, it follows that the potential at the centre of the cathode is less than that at the edges and thus the ordinary edge excess of potential is emphasised This is the explanation, I think, why sputtered deposit from the edges can be found in the central area of the cathode of an old X-ray bulb.

A deposit from the edges of the cathode forms also on the glass in the vicinity. This is objectionable and might possibly be prevented by constructing the cathode wholly free from sharp edges; the resulting greater uniformity of intensity might ensure that, with a soft tube at any rate, the active sputtering potential would not be reached.

Probably the point is of greater importance with the anticathode. No coil discharge is wholly free from the inverse current, and it is during this phase, when the anticathode officiates as cathode, that most of the mischievous blackening of an X-ray bulb occurs, the more especially with a metal like platinum. This might be largely got over by making the anticathode approximate in shape to a sphere.

ABSTRACT.

The Paper gives an account of the volatilisation of an aluminium cathode in a discharge tube containing helium. The sputtered deposit on the glass indicates that, under the conditions which prevailed, the disintegration was restricted to the edges of the cathode and did not occur elsewhere. Accordingly the complete outline of the cathode (made by rolling a sheet of aluminium into a nearly complete cylinder) was traced out by the deposit on the walls of the tube.

## DISCUSSION.

Prof. J. W. Nicholson asked if the size of the particles bore any relation to the atomic weight of the metal.

Prof. T. Mather asked if Dr. Kaye had made any tests of the conductivity of the films, and whether they obeyed Ohm's Law.

Mr. D. Owen inquired how adherent the films were to the glass.

The Author, in reply, stated that the size of the particles depended chiefly upon the nature of the gas in the tube. The method had been considerably used for preparing extremely high resistances though the film had to be annealed first. He believed the thinnest films did not obey Ohm's Law accurately. The adherence of the film varied with the metal. Gold adhered to glass well, but platinum was easily rubbed off till it had been heated to redness.

## XX. *On Vibration Galvanometers with Unifilar Torsional Control.* *By* ALBERT CAMPBELL, *B.A.*

RECEIVED APRIL 11, 1913. READ APRIL 11, 1913.

IN vibration galvanometers, whether of moving-circuit or moving-magnet type, the control torque employed has usually been either elastic or magnetic, or a combination of both. I am not aware that either gravity or electrostatic control has been utilised in any types. The following Table gives the outstanding features of the more familiar forms of vibration galvanometers :—

| Designation. | Type. | Current circuit. | Control. | Final tuning by |
|---|---|---|---|---|
| 1. Rubens, 1896 | Moving iron ... | Polar coils...... | Wire torsion and magnetic | Displacement of magnet poles |
| 2. Wien, 1901 ... | Moving magnet | Electromagnet | Ditto | Ditto |
| 3. Campbell, 1907 | Moving circuit | Moving coil... | Elastic (bifilar) | Alteration of wire tension |
| 4. Duddell, 1909. | Moving circuit | Moving loop ... | Ditto | Ditto |
| 5. Drysdale, 1910 | Moving iron ... | Fixed coil ...... | Magnetic | Alteration of field by magnetic shunt |

In order to obtain the full sensitivity in a vibration galvanometer it is essential to be able to vary the natural period of the vibrating system very gradually so as to get very fine tuning. It will be seen from the above Table that in different types the final tuning is done in a variety of ways. In the unifilar galvanometers of Rubens and Wien, although the control is largely due to the torsion of a wire, the final adjustment is made by slight alteration of the magnetic field. Some time ago I discovered that, if a single strip (of hard phosphor bronze) is used instead of a wire, the torsional control can be varied to a large extent by merely altering the tension of the strip. In fact the strip behaves like a bifilar suspension, and the tuning can be done with great exactness by first adjusting the effective length with a sliding bridge, and then making the fine adjustment by altering the tension.

I find that this property of certain strips, which has proved so useful, has also been noticed by Prof. L. R. Wilberforce, and investigated recently by Mr. H. Pealing ("Phil. Mag." p. 418, Vol. 25, March, 1913). Mr. Pealing gives examples of strips in which the torsional rigidity was increased by 40 to

50 per cent. by heavy loading; he finds that the anomalous effect is removed by annealing, and he attributes it to over-strains during the process of manufacture.

I have used the new system of unifilar tuning both in moving-coil and moving-iron vibration galvanometers, and find it very convenient. In the moving-coil type the coil is made quite detachable, having contact hooks at top and bottom, which are caught by minute hooks on the ends of the upper and lower strips. The sensitivity of such a galvanometer depends a good deal on the size of mirror which is considered sufficient. With a mirror of 15 sq. mm. area a sensitivity at 100 $\sim$ per second of 50 mm. at the meter distance per microampere is obtained, the effective resistance being about 700 ohms. Very good sensitivity can be obtained (at low frequencies) with strips of only 1 cm. to 2 cm. length, and the instrument thus can be of very moderate height.

The unifilar tuning is also successful for galvanometers of the moving-iron type. The moving iron may be in various forms—for example, in small permanent magnets (like Wien's) or in a polarised thin strip, as in one of Blondel's oscillographs. Mr. Pealing observed that continued tension for some weeks gradually diminishes the anomalous effect. I have had one galvanometer under full working tension for five or six weeks, and it is still behaving very well; but further observation on this point is desirable.

## ABSTRACT.

The Author exhibited a moving-coil vibration galvanometer in which a novel principle is used to obtain the fine adjustment of the control torque requisite for accurate tuning. He has found that in a phosphor-bronze strip under tension the torsional rigidity is considerably increased as the tension is raised. This anomalous behaviour of such strips has also been noticed by other observers (H. Pealing, "Phil. Mag.," March, 1913). If unifilar (strip) suspensions are used in a vibration galvanometer (whether of moving-coil or moving-iron type) the tuning can be done in just the same way as with bifilar suspension. In the moving coil instrument minute hooks on the ends of the strips engage in contact hooks at the top and bottom of the coil, which is easily detachable.

With a mirror of 15 sq. mm. area, at 100$\sim$ per second, a sensitivity of 50 mm. at a metre per micro-ampere can be obtained, the effective resistance being about 700 ohms.

## DISCUSSION.

Prof. T. MATHER asked Mr. Campbell if he did not think the variation of period was possible because of the bifilar behaviour of a flat strip.

Prof. C. H. LEES remarked that it seemed natural to expect a strip to behave like a bifilar. He thought Pealing's dismissal of this explanation would only be valid in the case of long strips.

The AUTHOR, in reply, stated that to some extent the result must certainly be due to bifilar behaviour. The effect due to this cause decreased as the length of the strip increased. In his own case where the length was only about 1 cm. it was probably large, but he did not think that this could be the whole explanation of the results in Pealing's experiment where a length of 40 cm. was used.

# XXI. *Interference of Röntgen Radiation (Preliminary Account).*

*By Prof.* C. G. BARKLA, *F.R.S., and* G. H. MARTYN, *B.Sc.*

RECEIVED APRIL 3, 1913. READ FEBRUARY 28, 1913.

FRIEDRICH, Knipping and Laue recently showed that when a narrow pencil of Röntgen radiation traverses a crystal many diffracted pencils proceed in various directions around the directly transmitted pencil from the portion of crystal traversed. Assuming the molecular structure of a crystal of zinc blende to be such as to form a space grating, they concluded that the images observed on the photographic plate would be produced if the radiation contained several conspicuous homogeneous constituents—that is radiation of several distinct wave-lengths.

Mr. W. L. Bragg pointed out that the directions of the pencils of radiation proceeding from the crystal could all be accounted for by considering simple reflection to take place at the various planes within the crystal containing the largest number of molecules, and that the radiation was analogous to white light giving a continuous spectrum in Röntgen radiation between certain limits of wave-length. The theory is in essentials the same as that of Laue, but it limits the applicability of Laue's theory in one direction and extends it in another.

We were engaged in investigating the phenomenon of the transmission of radiation through crystals, when an announcement of this important modification by W. L. Bragg was made. Two short notices were shortly afterwards published by us in "Nature." In the following Paper we now give a preliminary account of some of the experiments we have made up to the time of writing.

A crystal of rock-salt, which is of the simple cubic form, was placed with one set of cleavage planes horizontal, and one of the two sets of vertical cleavage planes in the geographical meridian say, consequently, with the other set in east west vertical planes. We will for convenience call the two sets of vertical planes the NS and the EW cleavage planes.

An X-ray tube was placed below the crystal and was arranged to move in circular grooves in a NS plane around a point in the small crystal as centre. The angle of incidence of the Röntgen radiation on the crystal could thus be varied, while the distance of the antikathode from the crystal remained constant.

*Reflexion of X-Rays.*—First the rays were allowed to pass through a very narrow circular aperture in a lead cap fixed to the bulb of the X-ray tube, and from this through a very narrow slit in a horizontal lead screen immediately below the crystal, the length of the slit being in a NS plane. Radiation passing through the slit was incident on the EW planes in the crystal at various angles as shown in the Fig. 1. It was found by

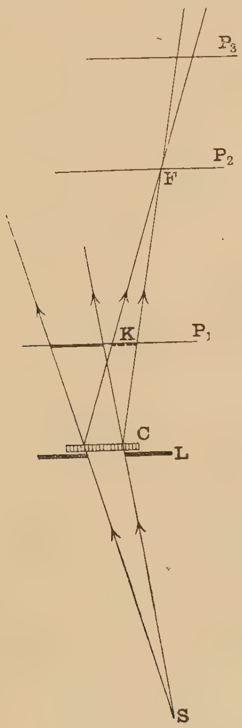


FIG. 1.

placing photographic plates at various distances above the crystal that the principal emergent pencil (excepting that directly transmitted) was one proceeding in the direction shown in the figure, indicating reflection from the EW cleavage planes. Thus the radiation from a point source diverging in a NS plane and falling on the EW cleavage planes produced a converging pencil which passed through a point focus (the vertical thickness of the crystal being negligible in comparison

with the other distances concerned). When the slit below the crystal was widened so that it became a wide rectangular aperture, photographic plates placed above the crystal showed a continual divergence of the reflected pencil in an EW plane, but a convergence in a NS plane, until the distance of the photographic plate above the crystal was approximately equal to that of the point source below. Beyond this it diverged in a NS plane. Thus at a distance above the thin crystal equal to that of the source below, the secondary radiation was brought to a line focus, the line being horizontal in an EW direction. When the incident pencil was rotated in the NS plane (the plane of the figure), the secondary pencil rotated in the same plane, but in the opposite sense through an equal angle approximately. When the incident pencil was turned from the N side to the S side of the EW cleavage planes, the secondary pencil also moved to the S side. In these experiments the angle of incidence varied only from about 80 deg. to 90 deg.

Thus an examination of the shape, direction and variation in direction of the secondary pencil show that this was a radiation regularly reflected from the cleavage planes within the crystal.

We must state here, however, that in later experiments when a much greater length of crystal was exposed, and the X-ray tube was removed to a much greater distance, the secondary beam did not converge to a line focus in the position we had expected on the simple reflection theory. Whether this was due to irregularity in the crystal structure or to the inadequacy of a simple reflection theory we cannot at present say. A tilt of cleavage planes through an angle of about 15 minutes would produce the deviation observed. We have, however, found the laws of regular reflection to hold with a fair degree of accuracy in our experiments with small crystal fragments when the angle of incidence was between 80 deg. and 90 deg.

*Fringe System.*—It has been seen that using a small source of radiation, and allowing a pencil to pass through a narrow slit with its length in a NS direction in the lead plate below the crystal, different portions of this pencil were incident at different angles on the EW cleavage planes. A photographic plate in position P, was affected in the position K by the radiations reflected at various angles, a definite point on the photographic plate corresponding to a definite angle of incidence and reflection. It was found that the narrow band produced on the plate with its length in a NS direction was broken up into a series

of approximately equal-spaced portions by maxima of photographic effect. At one end of the system there was complete separation of the maxima by portions of the plate which were not affected to any appreciable extent.

Such a system could be explained by interference between pencils of radiation reflected from a large number of equal-spaced EW cleavage planes in the crystal, the source being small, and the radiation producing photographic action being fairly homogeneous, that is consisting of waves of length within fairly narrow limits. This was originally suggested by us as a possible explanation which seemed adequate to the phenomena we had observed. Later experiments have not given support to this simple explanation, though the phenomena still appear to be due to interference.

On removing the lead diaphragm immediately outside the X-ray tube, and thus exposing the crystal to the whole of the antikathode, the source was, of course, more extended. The result was in some cases the almost complete disappearance of the above fringe system due to overlapping. When, however, the antikathode was situated at a considerable distance from the crystal the fringes were still quite clearly marked, as the source was again small compared with the distance from the crystal.

Up to the time of writing five crystal fragments have been used (two, however, being from the same large crystal) and these fringes have been observed with each. In some cases the fringe system became blurred owing to obvious irregularities in the crystal, and in parts it was indistinguishable.

The most extended system observed was one in which at one end of the image the fringes were very distinctly separated, while at the other they appeared to coalesce in pairs. Assuming each fringe at the latter end still to contain two constituent fringes, the full series consisted of 26 fringes.

*Band System.*—Using a long rectangular aperture it was seen that the rectangular image obtained by reflection as before was broken up into a system of bands separated by spaces showing little or no photographic action. These spaces coincided in position with minima of the fringe system, while each band of this second larger system usually contained eight complete fringes of the first system. (These appeared as four in the coalesced form.) That is, a long rectangular image was divided into a number of bands of approximately equal length, each band being very definitely separated from the next, and

each containing a number of fringes which were usually much less clearly marked.

In addition to the fringe system and band system above described, there was a variation in intensity of the image from band to band. A sufficiently large bumber of bands has not been observed to enable us to see if this variation in intensity is regularly periodic, that is if there is a larger system of bands, containing the system described above, Not more than six bands have been observed in one image.

*Brushes.*—Other pencils of radiation not obeying the laws of regular reflection have been found to be emitted by some crystals. Those are shown on the photographic plate by brushes spreading out from the image due to the directly transmitted radiation. These are thus due to pencils of radiation apparently spreading out in planes passing through the direction of propagation of the primary radiation.

Though sufficient experiments have not been performed to enable us to arrive at a final conclusion as to the exact origin of the phenomena observed, it seems desirable that the experimental results should be recorded at the present time, for the full investigation will probably be somewhat prolonged, and the results already arrived at are of interest.

Examination of the photographic effect of the beam reflected from the NS planes—obtained by giving these a small angle with the vertical—revealed no variation in intensity from end to end of the band. There was no indication either of the bands or the fringes running EW, which were so clearly marked in the photographic effects of the pencils reflected from the EW cleavage planes. Thus of the two reflected beams, reflected presumably from the same molecules, one exhibited periodic variation in intensity from end to end, while the other showed no such variation. The latter, however, showed fringes parallel to its own NS reflecting planes. We cannot at present account for this except by interference of one form or another. If it turns out to be due simply to a periodic change of angle of the reflecting planes the interference is of the kind resulting in regular reflection, but shown by this difference in the structure of the images in a more striking way than usual.

The beam of radiation directly transmitted through the crystal, though different portions of it passed through at different angles, produced a photographic image which even when of the same average density as that of the image due to reflection, exhibited absolutely no variation in intensity from end to

end. Thus the variation in intensity was in no way due to a variation in absorption of the incident or the reflected radiation.

The source of radiation was moved in a NS plane, so that the angle of incidence on the EW planes changed by about 5 deg. The whole reflected system moved through approximately the same angle. The bands and fringes shown on the photographic plate moved bodily along, the maxima and minima appearing to originate in precisely the same portions of the crystal as previously.

The crystal was moved horizontally in a NS plane over the rectangular aperture, while the source of radiation was fixed. The bands and fringes moved with the crystal, again indicating that maxima and minima of intensity originated in the same positions as previously.

The crystal was rotated about a vertical axis through 180 deg. The features of the band and fringe systems appeared to have turned through 180 deg., the positions being identified by irregularities in the bands and fringes.

Thus the features appeared fixed with respect to the crystal, that is the features *originated* at definite parts of the crystal, at any rate under the stated changes of experimental condition.

Corresponding features were shown, too, in the images obtained by reflection in other vertical planes, as, for instance, the vertical planes running north-west to south-east, and again the maxima and minima appeared to originate in the same parts of the crystal as the corresponding maxima and minima in the other images. In these cases, however, the distance traversed by a ray from one reflecting plane to the next was the same as in the case of the EW planes. The contrast with the image due to reflection in the NS planes was most marked.

As stated above, several crystals were used, and when the other conditions of the experiments were kept constant little variation in fringe-width was observed. A variation in band-width of greater magnitude however was observed, a certain crystal 5 mm. thick, producing bands 25 per cent. smaller than the case of another crystal 2 mm. thick. In these experiments the fringes were not visible, and the bands not so well marked as in later experiments. The possible causes are too numerous to speculate upon at this stage.

The thick crystal was turned through 90 deg. about a vertical axis, so that the previous NS planes become the principal reflecting planes. The band-width was found as closely as measurable the same as in the first position.

The penetrating power of the primary radiation was varied considerably, but when care was taken to keep the other conditions of the experiment as nearly as possible the same, no variation in band-width was observed.

The quality of the radiation in the reflected pencil was tested, first by placing absorbing sheets of aluminium in its path and observing the density of the photographic image. The reflected radiation was found to be of a penetrating type. It was neither corpuscular (electronic) radiation nor characteristic (fluorescent) X-radiation, but might be of the quality of the only other type of secondary radiation known—scattered X-radiation. By the ionisation method* it was found that the radiation constituting the reflected pencil (together with a scattered radiation, the intensity of which was scarcely sufficient to produce appreciable photographic effect) was comparatively homogeneous. The same was true of the directly transmitted pencil of X-radiation, which it closely resembled in penetrating power. It was probably no more homogeneous than any ordinary beam of Röntgen radiation becomes by transmission through a thick sheet of substance by selective absorption of the longer waves.† The intensity of a reflected pencil was measured by comparing the times required by this and by the directly transmitted pencil to produce equal photographic effects. It was found that the intensity of the reflected pencil was only $\frac{1}{1500}$ of that of the directly transmitted pencil. This is only a very small fraction which may be roughly estimated as of the order $\frac{1}{200}$ of the total energy of the radiation scattered by the rock-salt. [We should expect regular reflection to take place from comparatively fixed electrons, whereas scattering might be produced by both fixed and moving electrons.]

Finally, we have not yet arrived at any definite conclusion regarding the exact origin of the periodic variation in intensity of the reflected beam. It appears to be due to a corresponding

* W. H. Bragg first showed that the reflected radiation produced ionisation in air, while Moseley and Darwin found in a certain case the penetrating power of the radiation was very like that of the primary radiation.

† We should like here to take the opportunity of correcting some misapprehensions that arose in the discussion on this Paper. One of us said that the *fluorescent X-radiation from an element* was quite probably as homogeneous as the radiation giving some spectral lines—say as homogeneous as the light from a sodium flame; that the homogeneity of the X-radiation here dealt with was, however, only such homogeneity as would result from a selective absorption of the longer waves from a radiation originally giving a continuous spectrum between certain limits. How homogeneous this was we were not then prepared to say.

large structure in the crystal itself. If there were a periodic break in crystalline structure corresponding features might be expected to be shown by reflection in the NS planes as well as by reflection in the EW planes. We have seen no indication of this. Again a periodic progressive change of angle of the cleavage planes would result in a separation of the reflected pencils such as we have not observed. A periodic change of angle alternating in direction would produce two systems of fringes the superposition of which in some positions would produce the fringe systems observed. It will be necessary to carefully trace the directions of these fringes from the crystal source outwards in order to determine their exact method of propagation. Such twinning, however, cannot as far as we see at present account for the separation into bands.

The fact that the features disappear when the radiation proceeds in certain directions is evidence of interference.

The investigations are being continued. Neither the variation in intensity of the bands nor the origin of the brushes of radiation has yet been studied by us.

NOTE.—I wish to thank the Solvay International Institute for a grant in aid of these researches, and Dr. Sibly for a number of crystal specimens and for his kindness when I have consulted him on crystallography.—C. G. B.

ABSTRACT.

The authors have made a preliminary investigation of the Röntgen radiation proceeding from a crystal of rock salt (which is of the simple cubical form) when a pencil of Röntgen radiation is incident in a direction nearly grazing one of the three sets of mutually perpendicular cleavage planes.

*Reflection of X-rays by the Cleavage Planes.*—Using a very narrow pencil of radiation, it was seen that the principal secondary pencil was one obeying the laws of reflection from the cleavage planes.

A pencil diverging in all directions from a point source produced a corresponding reflected pencil of radiation converging to a line focus after reflection from a set of parallel cleavage planes. The quality of the radiation forming the secondary pencils was shown both by the photographic and by the ionisation method to be, not the fluorescent X-radiation, but of the kind previously described as scattered X-radiation. It was approximately of the same penetrating power as the primary radiation, and was approximately homogeneous, having traversed 5 mm. of rock salt in the case investigated.

*Interference Fringe Systems.*—A diverging pencil of radiation was directed on to a crystal so that various portions were incident on the cleavage planes at different angles. A photographic plate showed the relative intensity of the corresponding reflected radiations. It was seen that the intensity of the reflected pencil varied periodically

with varying angle of incidence, the maximum being separated by intervals corresponding to approximately equal increments in the value of cos $\theta$, where $\theta$ was the angle of incidence on the reflecting planes.

Such a series of maxima may be explained by interference of the pencils reflected from equal spaced parallel planes, the maxima being spectra of various orders.

The wave-length, calculated on the assumption that these are planes passing through corresponding portions of molecules in the planes of cleavage, and that a molecule is simply NaCl, is found to be $0 \cdot 6 \times 10^{-9}$ cm. If the molecule be more complex, the calculated wave-length would be greater. This value thus agrees remarkably well with the value (between 1 and $2 \times 10^{-9}$ cm.) calculated from the velocity of ejection of electrons by this X-radiation, taking this to behave as ultra-violet light of short wave-length.

Using a more extended source the above "spectral lines" became indistinct and disappeared, but there appeared a periodic variation in intensity, the band-width being about four times the distance between the above lines. Such a system of interference bands would be produced by a second plane of reflection within the molecules in a position dividing the distance between the other planes in the ratio 1 : 3 approximately. All the evidence considered indicates that these effects are due to interference. Various crystals were used, and one was turned through a right angle so that another system of planes acted as reflecting planes.

The only experiment made up to the time of writing on the effect of varying the penetrating power showed a 25 per cent. smaller band-width with a radiation of increased penetrating power. Further experiments are, however, being made.

Finally, there can be little doubt that the fringe systems are interference fringe systems. That the smaller system is a series of spectra of different orders and the other an interference band system seems probable; this theory certainly explains the results observed up to the time of writing.

## DISCUSSION.

The PRESIDENT remarked that Prof. Barkla, in explaining the second system of fringes, assumed that each molecule contained an excentrically placed electron similarly situated, and that all these electrons were stationary, which was contrary to the usual ideas on the subject. Prof. Barkla also rather sharply distinguished between regular reflection and scattering. But regular reflection could be considered to be due to a system of scattered wavelets.

Prof. C. H. LEES thought it was surprising to see that the results were capable of such a simple explanation, and hoped that such experiments would lead to more knowledge of the structure of the molecules.

Prof. J. W. NICHOLSON pointed out that if the molecule had been assumed to be more complex than NaCl the wave-length obtained would have been greater.

Dr. G. W. C. KAYE remarked that these experiments constituted a fresh blow to the corpuscular theory of the X-rays. The present experiments showed a great similarity between X-rays and ordinary light. Had Prof. Barkla used other crystalline substances? He hoped that the results obtained with rock salt could be generalised from.

Mr. C. E. S. PHILLIPS inquired about the time of exposure necessary.

Had Prof. Barkla made experiments with mica or star-mica? Some experiments with these substances made at the Cancer R search Hospital showed irregularities they could not easily explain.

Mr. D. Owen remarked that the experimental results were interpreted by the author as implying the existence of homogeneous etheric waves. It seemed, however, unthinkable that radiation of definite wave-length could be emitted from the anti-cathode of the X-ray bulb, considering that the exciting cause of the production of these rays was the bombardment of the metal by cathode-ray particles. The values deduced for the wave-length were of the same order as the dimensions of the molecule. This result suggested that the definiteness of structure of the rays was imparted by the arrangement of the molecules in the crystal employed, that this definiteness existed only *after* the crystal had produced its effect, and that what was being measured was not the wave-lengths of the incident radiation but the distance apart of the molecules. Might not the phenomena be then of the same kind as those occurring in the "palisade effect" in acoustics? It would be of interest to know whether the wave-lengths found depended on the nature of the X-ray bulb, whether "hard" or "soft."

Prof. Barkla, in reply, stated that by chemists it was considered highly probable the molecule was more complex than NaCl. On the theory given, the wave-length would be proportional to the cube root of the number of atmos in the molecule; thus, if a molecule were $Na_8Cl_8$, this would make the calculated wave-length twice as great. When the full anti-cathode was used the exposure given was four or five hours. With a restricted pencil the exposures given sometimes amounted to 18 hours. The crystal plates employed were 2 mm. to 5 mm. thick. After passing through this plate the rays were very homogeneous. One of the things that the corpuscular theory of X-rays had been put forward to explain was the fact that X-rays of a given quality, when absorbed by any material, caused the emission of electrons, the maximum velocity of which depended solely on the quality of the X-rays, and not on their intensity. But the same was true of light, the wave nature of which there was little disposition to question.

Note.—The results of further experiments are given in the Paper.

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